

Space Studies Board

Annual Report

1995

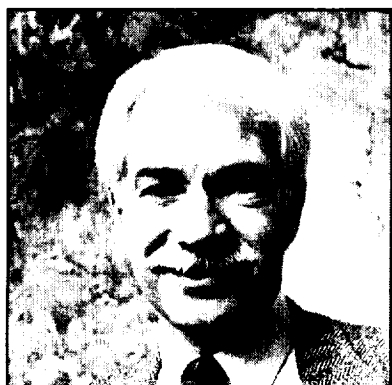
NATIONAL RESEARCH COUNCIL

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The Space Studies Board is a unit of the National Research Council, which serves as an independent advisor to the federal government on scientific and technical questions of national importance. The Research Council, jointly administered by the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine, brings the resources of the entire scientific and technical community to bear through its volunteer advisory committees.

Support for the work of the Space Studies Board and its committees and task groups was provided by National Aeronautics and Space Administration contract NASW-4627, National Oceanic and Atmospheric Administration contract 50-DGNE-5-00210, and Naval Research Laboratory Grant N00014-94-1-G031.

From the Chair



It is becoming hard to remember a time when the space program was in what might be called a “steady state.” Indeed, it has been in various conditions of uncertainty and flux for well over a decade, arguably ever since the end of the Apollo program. But even so, the transformations now under way are truly profound.

The activities of the Space Studies Board and its committees in 1995 highlight some of the recent changes affecting the content and conduct of space research across its many disciplines. A few reports were commissioned specifically to deal with some significant change or wrenching choice driven primarily by budgetary pressures. Even those others that address more traditional, scientific questions carry clear imprints of current political and fiscal realities.

Completed in September, *Managing the Space Sciences* reports the results of the year’s most comprehensive study on the conduct of space research. This document, taken together with several short reports on particular questions of science management and an account of an experiment in priority-setting methodology, offers fundamental principles and detailed suggestions on how NASA can best carry out its charter in space science and applications. Responding to congressional and NASA requests, *Managing the Space Sciences* examines the organization of science management at NASA, provides guidelines for the very difficult process of setting priorities among and across distinct disciplines, and analyzes mechanisms for ensuring the insertion of new technologies (this last component included participation by the National Research Council’s Aeronautics and Space Engineering Board). NASA appears very receptive to the report and its recommendations.

The climate for space research was clearly a factor in several reports dealing with specific research areas or missions. *Earth Observations from Space: History, Promise, and Reality* is retrospective of the ever changing civil remote sensing program over the past 15 years, spanning NASA, NOAA, and DOD programs and activities. It was written with the hope that future programs would benefit from lessons learned by studying the past. In the report *Review of Gravity Probe B*, the Board responded to an urgent request for a scientific and technical evaluation of this ongoing project, including an unprecedented assessment of its comparative scientific value. *The Role of Small Missions in Planetary and Lunar Exploration* is an analysis of how the current trend toward lighter spacecraft meshes with scientific priorities in solar system research. Possible opportunities for scientific use of defense-sponsored space system development are discussed in the Board’s *Scientific Assessment of a New Technology Orbital Telescope*.

All the Board’s reports must be grounded in basic analysis of research priorities and scientific opportunities best addressed through the space program. In two disciplines, the relatively new area of microgravity research and the more traditional field of space physics, the Board and its committees published new research strategies that lay out

the most important topics for the coming years. A shorter and more narrowly focused report treats the scientific value of Earth observations by the Shuttle Radar Laboratory.

All indications are that the Board's guidance is used by those policy makers who must finally make the hard decisions about what we should do in space and how we should do it. This is the real reward for the hundreds of scientists who, working with dedicated NRC staff, volunteered their days, evenings, and weekends during 1995 to produce the reports presented in these pages.

A handwritten signature in black ink, reading "Claude R. Canizares". The signature is fluid and cursive, with the first name "Claude" being the most prominent.

Claude R. Canizares
Chair
Space Studies Board

April 1996

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1

Charter and Organization of the Board

THE ORIGIN AND FOUNDING CHARTER OF THE SPACE SCIENCE BOARD

The National Academy of Sciences was chartered by the Congress, under the leadership of President Abraham Lincoln, to provide scientific and technical advice to the government of the United States. Over the years, the advisory program of the institution has expanded, leading in the course of time to the establishment of the National Academy of Engineering and the Institute of Medicine, and of the National Research Council (NRC), today's operational arm of the Academies of Sciences and Engineering.

After the launch of Sputnik in 1957, the pace and scope of U.S. space activity were dramatically increased. Congress created the National Aeronautics and Space Administration (NASA) to conduct the nation's ambitious space agenda, and the National Academy of Sciences created the Space Science Board. The original charter of the Board was established in June 1958, 3 months before final legislation creating NASA was enacted. The Space Science Board has provided external and independent scientific and programmatic advice to NASA on a continuous basis from NASA's inception until the present.

The fundamental charter of the Board today remains that defined by National Academy of Sciences President Detlev W. Bronk in a letter to Lloyd V. Berkner, first chair of the Board, on June 26, 1958:

We have talked of the main task of the Board in three parts—the immediate program, the long-range program, and the international aspects of both. In all three we shall look to the Board to be the focus of the interests and responsibilities of the Academy-Research Council in space science; to establish necessary relationships with civilian science and with governmental science activities, particularly the proposed new space agency, the National Science Foundation, and the Advanced Research Projects Agency; to represent the Academy-Research Council complex in our international relations in this field on behalf of American science and scientists; to seek ways to stimulate needed research; to promote necessary coordination of scientific effort; and to provide such advice and recommendations to appropriate individuals and agencies with regard to space science as may in the Board's judgment be desirable.

As we have already agreed, the Board is intended to be an advisory, consultative, correlating, evaluating body and not an operating agency in the field of space science. It should avoid responsibility as a Board for the conduct of any programs of space research and for the formulation of budgets relative thereto. Advice to agencies properly responsible for these matters, on the other hand, would be within its purview to provide.

Thus, the Board exists to provide advice to the federal government on space research, and to help coordinate the nation's undertakings in these areas. With the reconstitution of the Board in 1988 and 1989, the Board assumed similar responsibilities with respect to space applications. The Board also addresses scientific aspects of the nation's program of human spaceflight.

THE 1988 REORGANIZATION OF THE BOARD—THE SPACE STUDIES BOARD

In 1988, the Space Science Board undertook a series of retreats to review its structure and charter. These retreats were motivated by the Board's desire to more closely align its structure and activities with evolving government advisory needs and by its assumption of a major portion of the responsibilities of the disestablished NRC Space Applications Board. As a result of these retreats, a number of new task groups and committees were formed, and several existing committees were disbanded and their portfolios distributed to other committees. In addition, since civilian space research now involves federal agencies other than NASA (for example, the National Oceanic and Atmospheric Administration (NOAA), the Departments of Energy and Defense, and the National Science Foundation (NSF)), it was decided to place an increased emphasis on broadening the Board's advisory outreach.

MAJOR FUNCTIONS

The Board's overall advisory charter is implemented through four key functions: discipline oversight, interdisciplinary studies, international activities, and advisory outreach.

Oversight of Space Research Disciplines

The Board has responsibility for strategic planning and oversight in the basic subdisciplines of space research. This responsibility is discharged through a structure of standing discipline committees, and includes preparation of strategic research plans and prioritization of objectives as well as assessment of progress in these disciplines. The standard vehicle for providing long-term research guidance is the research strategy report, which has been used successfully by the Board and its committees over many years. In addition, committees periodically prepare formal assessment reports that examine progress in their disciplines in comparison with published Board advice. From time to time, in response to a sponsor or Board request or to circumstances requiring prompt and focused comment, a committee may prepare and submit a short, or "letter," report. Agency requests for broader space policy or organizational advice are addressed by suitable ad hoc organizational arrangements and appropriate final documentation. Other special agency requests that require responses synchronized with the federal budget cycle are relayed to standing committees for action or are taken up by ad hoc task groups. All committee reports undergo Board and NRC review and approval prior to publication and are issued formally as reports of the Board.

Individual discipline committees may be called upon by the Board to prepare specialized material for use by either the Board or its interdisciplinary committees or task groups.

Interdisciplinary Studies

Although the emphasis over the years has been on discipline planning and evaluation, the reorganization of the Board recognized a need for cross-cutting technical and policy studies in several important areas. To accomplish these objectives, the Board creates internal committees of the Board and ad hoc task groups. Internal committees, constituted entirely of appointed Board members, are formed for short-duration studies, or lay the planning groundwork for subsequent formation of a regular committee or task group. Task groups resemble standing discipline committees in structure and operation, except that they have predefined lifetimes, typically one to three years, and more narrowly bounded charters.

International Representation and Cooperation

The Board continues to serve as the U.S. National Committee for the International Council of Scientific Unions (ICSU) Committee on Space Research (COSPAR). In this capacity, the Board participates in a broad variety of COSPAR panels and committees.

In the past, COSPAR bylaws have provided that its two vice presidents be from the United States and the USSR. The U.S. Vice President of COSPAR has served as a member of the Board, and a member of the Board's staff has served as executive secretary for this office. During 1994, governance of COSPAR evolved to fully democratic election of officers. The Board continues as the U.S. National Committee, but its representation within the COSPAR officer corps is now determined electorally.

As the economic and political integration of Europe evolves, so also does the integration of Europe's space activities. The Board has successfully collaborated with the European space research community on a number of ad hoc joint studies in the past and is now seeking in a measured way to broaden its advisory relationship with this community. The Board has established a regular practice of exchanging observers with the European Space Science Committee (ESSC), an entity of the European Science Foundation. Strengthening contacts with the Russian and Japanese programs is expected to assume higher priority as contacts with European research mature.

Advisory Outreach

The Space Science Board was conceived to provide space research guidance across the federal government. Over the years, the Board's agenda and funding have focused on NASA's space science program. Since the Board's reorganization, however, several influences have acted to expand the breadth of the Board's purview, both within NASA and outside it.

First, the incorporation of scientific objectives into manned flight programs such as the shuttle and space station programs dictates additional interfaces with responsible offices in NASA. The Board is strengthening its links to the Office of Space Access and Technology in NASA through joint activities with the NRC's Aeronautics and Space Engineering Board. Formal contacts may be made with NASA's space operations, international affairs, and commercial offices and programs.

Second, the assumption of the space applications responsibilities from the dissolved NRC Space Applications Board has implied a broadening of the sponsorship base to NOAA, with its responsibilities for operational weather satellites. In response, NOAA became a cosponsor of the Board's Committee on Earth Studies in 1991 and continued this advisory relationship in 1995.

Third, the maturation of some of the physical sciences has led to progressive integration of space and nonspace elements, suggesting a more highly integrated advisory structure. One example is the solar-terrestrial community, where the Board's Committee on Solar and Space Physics has operated for several years in a "federated" arrangement with the NRC Committee on Solar-Terrestrial Research. Another example is astronomy, where the Board operates a Committee on Astronomy and Astrophysics as a joint committee of the Space Studies Board and the Board on Physics and Astronomy. An area of possible future disciplinary association is between the National Institutes of Health and space biology research.

With the end of the Cold War, new participants will become involved in areas of space research previously exclusively civilian. In 1993, the Board established partial support for the Committee on Planetary and Lunar Exploration by the Strategic Defense Initiative Organization and performed an initial assessment of the Clementine mission to the Moon and an asteroid. This convergence, which is also taking place in other areas of the federal R&D establishment, is coming about partly because of shared technology interests and partly because of declassification of some defense technologies in response to the changing world geopolitical environment. The Ballistic Missile Defense Organization (BMDO) has considered several space missions of potential scientific interest, including a large-aperture infrared telescope. As a result, the Board continued its sponsorship and advisory relationship with the BMDO by initiating a scientific assessment of this telescope proposal.

In summary, the Board will continue to reach out to nonresearch NASA offices and to other federal agencies, seeking to establish advisory and corresponding sponsorship relationships as appropriate.

ORGANIZATION

The Board conducts its business principally during regularly scheduled meetings of its own membership and of its supporting committees. These include the internal committees of the Board, standing discipline committees, and ad hoc task groups. During 1995, the Board also managed a major policy study entitled "The Future of Space Science"; this project was executed by a network of ad hoc task groups and an augmented Joint Committee on Technology. The organization of the Board and its committees during 1995 is illustrated in Figure 1.

The Space Studies Board

The Space Studies Board is composed of 20 to 24 prominent scientists, engineers, industrialists, scholars, and policy experts in space research, appointed for staggered terms of one to three years. The Board meets three or four

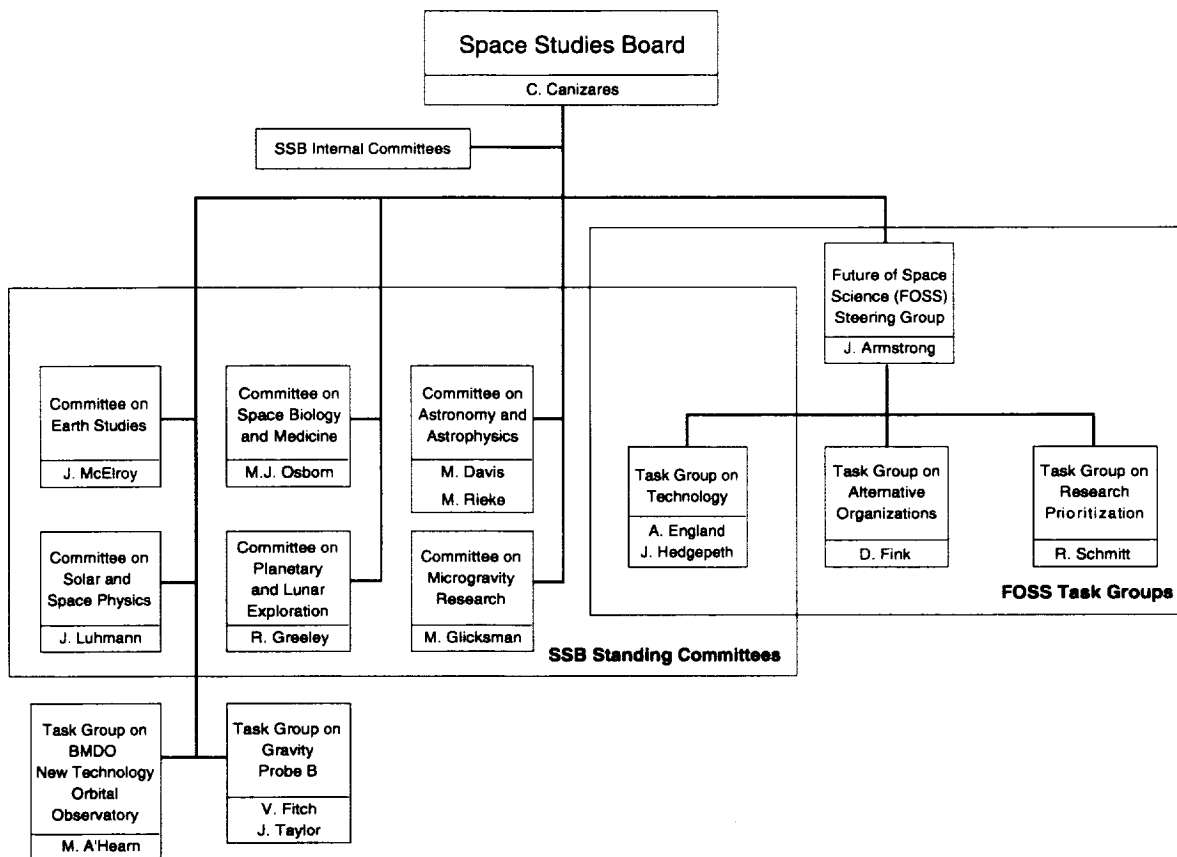


FIGURE 1 Organization of the Space Studies Board and its committees during 1995.

times per year to review the activities of its committees and task groups and to be briefed on and discuss major space policy issues. The Board is constituted in such a way as to include as members its committees' chairs; other Board members serve on internal committees of the Board or perform other special functions as designated by the Board Chair. The Board seats, as ex officio members, the chairs of the NRC Aeronautics and Space Engineering Board and of the NRC Naval Studies Board's Space Panel.

In general, the Board develops and documents its views by means of appointed discipline committees or interdisciplinary task groups that conduct studies and submit their findings for Board and NRC approval and dissemination. These committees or task groups may collaborate with other NRC boards or committees in order to leverage existing specialized capabilities within the NRC organization. On occasion, the Board itself deliberates cross-cutting issues and prepares its own statements and positions. These mechanisms are used to prepare and release advice either in response to a government request or on the Board's own initiative. In addition, the Board comments, based on its publicly established opinions, in testimony to Congress.

Internal Committees of the Board

Internal committees facilitate the conduct of the Board's business, carry out the Board's own advisory projects, and permit the Board to move rapidly to lay the groundwork for new study activities. Internal committees are composed entirely of Board members, who generally serve for 1 to 2 years and then are rotated for replacement by other Board members. Internal committees active during 1995 included the Executive Committee of the Board (XCOM) and the Committee on International Programs (CIP). The Joint Committee on Technology (JCT) was expanded with non-Board members to help carry out a special study, described further below. The Committee on

Human Exploration (CHEX), previously a regular standing committee of the Board, was inactive pending further maturation of national human spaceflight goals.

Discipline Committees

The standing discipline committees form the traditional backbone of the Board and are the means by which the Board conducts its oversight of space research disciplines. Each discipline committee is composed of 10 to 16 specialists, appointed to represent the broad sweep of research areas within the discipline. In addition to developing long-range research strategies and formal program and progress assessments in terms of these strategies, these committees perform analysis tasks in support of interdisciplinary task groups and committees, or in response to other requirements as assigned by the Board. In 1995, there were six discipline committees:

- Committee on Astronomy and Astrophysics (CAA)
- Committee on Earth Studies (CES)
- Committee on Microgravity Research (CMGR)
- Committee on Planetary and Lunar Exploration (COMPLEX)
- Committee on Solar and Space Physics (CSSP)
- Committee on Space Biology and Medicine (CSBM)

Activities of the former Committee on Space Astronomy and Astrophysics were terminated in 1989 when the Astronomy and Astrophysics Survey Committee began its work. A new Committee on Astronomy and Astrophysics (CAA) was established in 1992 and tasked with resuming oversight of NASA's space astronomy program. The CAA is operated jointly with the NRC Board on Physics and Astronomy, for which it performs oversight of ground-based research programs under sponsorship from the NSF.

The CSSP continued to operate in a "federated" arrangement with another NRC committee, the Committee on Solar-Terrestrial Research of the Board on Atmospheric Sciences and Climate. While the two committees retained their separate identities and reporting relationships to their parent boards, they continued to meet jointly, submitting study results to whichever of the respective boards sponsored a given activity.

Project on the Future of Space Science

Under various pressures, the nation's civil space research program conducted by NASA for over 35 years is undergoing sweeping change. Space science has in many areas successfully completed its initial reconnaissance phase. At the same time, the national imperative to control the deficit has dimmed prospects for future funding growth. In March 1993, a reorganization of NASA eliminated the Office of Space Science and Applications (OSSA), which had theretofore performed agency-wide science mission and program planning. In response to the likelihood of constrained future budgets and the consequent need for careful selection and efficient execution of space science missions, the Senate Subcommittee on VA, HUD, and Independent Agencies provided, under the title "Future of Space Science" (FOSS), for the National Academy of Sciences to undertake studies in several germane areas.

Responding to a subsequent request by NASA Administrator Daniel Goldin, the Space Studies Board undertook an assessment of the role and position of space science within NASA. This assessment focused on specific areas identified in the administrator's request and in the earlier FY94 Senate appropriations report language. These areas are the organization of civil space research programs within the agency, merit-based cross-disciplinary prioritization, including preservation of innovative initiatives, and improvement of technology utilization in science missions.

The adopted approach to carrying out the requested study was to use the Space Studies Board's in-place advisory structure wherever possible. The Board formed a FOSS Steering Group, two new task groups, and adapted its existing Joint Committee on Technology (JCT) for the project. The chairs of the FOSS steering group and supporting task groups were appointed to the Board. Some current Board members served as liaison members of the Steering Group and task groups. In addition, the Board's six standing space research discipline committees were tasked to support the study.

The following four topics were explicitly specified in the legislative report and the Administrator's request:

- Alternative organizational models for space science,
- Analysis of merit-based prioritization,
- Improvements in technology insertion, and
- Enabling innovative research.

The second and fourth topics are very closely related: a merit-based prioritization scheme must make special provisions for support of unproven research areas if fostering and preserving such research are to be an outcome of the science selection process. Based on analysis of the Senate language and the NASA Administrator's request, the Board established a four-component study organization:

- Steering Group (FOSS-SG),
- Task Group on Alternative Organizations (FOSS-AO),
- Task Group on Research Prioritization (FOSS-RP), and
- Task Group on Technology (FOSS-T) (JCT).

The distribution of study tasks among these FOSS panels is described in the "Program" section below. The task groups completed their work, and a final report was delivered to NASA in September.

Task Groups

Ad hoc task groups are created by Board action with NRC approval.

Formed during the 1988 reorganization of the Board, the Task Group on Priorities in Space Research has completed its study and been dissolved. Its final report was released in 1995.

The Committee on Astronomy and Astrophysics established the Panel on Optical and Infrared Astronomy, which examined management issues in ground-based astronomy for the Board on Physics and Astronomy under the sponsorship of NSF. This report was completed and released early in 1995.

In mid-1994, the Space Studies Board formed the Task Group on the BMDO New Technology Orbital Observatory (TGBNTOO) in response to a request by the BMDO. The final report of this task group was completed and issued in mid-1995.

During the final months of 1994, the NRC received a request from NASA Administrator Goldin to perform an assessment of the scientific merit and technical feasibility of the Gravity Probe B (GP-B) mission. Working with the Board on Physics and Astronomy, the Space Studies Board established the Task Group on Gravity Probe B to conduct the required study. The final report was completed and delivered in May 1995, and the task group was disbanded.

2

Activities and Membership

During 1995, the Space Studies Board and its committees and task groups gathered for a total of 40 meetings. The following narratives present highlights of these meetings. Formal study reports and short reports developed and approved during the meetings and issued during 1995 are represented in this annual report either by their executive summaries (for full-length reports) in Section 3 or by reproduction in full (for short reports) in Section 4.

Nine full-length reports were distributed or delivered, including a congressionally mandated report by the Committee on the Future of Space Science (executive summary in Section 3.6) and a comprehensive survey of Earth observation programs by the Committee on Earth Studies (Section 3.4). Major research guidance reports were completed and published by the Committee on Microgravity Research (3.2) and by the federated Committee on Solar and Space Physics/Committee on Solar-Terrestrial Research (3.9). Several significant assessment reports were also published, including an assessment of small missions by the Committee on Planetary and Lunar Exploration (3.7), a scientific evaluation of Gravity Probe B by the Task Group on Gravity Probe B (3.3), and an analysis of technologies for a 4-meter active optics telescope by the Task Group on BMDO New Technology Orbital Observatory (3.5). In addition, the Committee on Astronomy and Astrophysics' Panel on Ground-based Optical and Infrared Astronomy released its report (3.1), and the Task Group on Priorities in Space Research issued its second and final report (3.8).

Five short reports were also prepared and released during 1995. They addressed such diverse topics as reflight of shuttle-borne synthetic aperture radars, the role of NASA centers and center scientists in scientific research, guidelines for establishment of NASA research institutes, and clarification of findings of the microgravity research opportunities report and of the Future of Space Science Committee's management study. These short reports are reprinted in full in Section 4.

SPACE STUDIES BOARD

Prepared during the first months of last year, our 1994 annual report noted a clearer emergence of "the post-Cold War evolution of the national policy and budget environment first heralded a year ago." While those budget trends continued throughout 1995, the policy climate for space and space research was being revolutionized. The pressures to downscale that NASA and other federal agencies faced in 1994 continued to intensify, as represented by the Administration's Reinventing Government-2 (REGO-2), for example, and the drive toward a balanced budget by the new Republican Congress. For research, potent political forces countermanded earlier pressures toward "strategic" research and began to reemphasize basic research. Some formerly favored programs, such as the Department of Commerce's Advanced Technology Program, came to be perceived as unwarranted intrusions by the government into properly private sector decision making—"pubescent industrial policy," in the words of one influential congressman.

For NASA, the big budget news during the first quarter of 1995 was the additional \$5 billion of out-year reductions mandated in mid-January by the White House to pay for proposed tax cuts. In a very dramatic budget press conference on February 6, Administrator Daniel Goldin stated: "Make no mistake. When this is over, NASA will be profoundly different. We're going to restructure the Agency." Mr. Goldin made clear that his preferred solution was to reduce NASA's infrastructure and work force, rather than delete programs. Nonetheless he admitted, "This will be painful. It means NASA employees will lose their jobs. Contractors working at NASA and at their own offices will lose their jobs." That this was not empty rhetoric became increasingly apparent. In mid-March, Mr. Norman Augustine, president of the new aerospace behemoth Lockheed Martin, announced the beginning of a company consolidation process projected to lead to elimination of 25,000 to 40,000 of its 170,000 jobs. On March 27, the Clinton Administration revealed REGO-2, itself intended to eliminate about 13,000 jobs by the end of the decade. While most of the downsizing would take place in the private sector, NASA headquarters itself was targeted for a 40% reduction by the end of the decade, with the field centers facing smaller but still very substantial cuts.

Several NASA management studies were completed during the first quarter, including the "NASA Federal Laboratory Review" (by the Foster Committee) and reviews of shuttle operations. A "red team white paper" prepared for discussion by NASA management generated anxiety and discussion with its call for widespread consolidation of agency functions. The pace of meetings of the Board's own Future of Space Science (FOSS) project accelerated during the quarter, with NASA Headquarters-field center roles and relationships coming under new scrutiny.

On the science front in early 1995, NASA's Solar System Exploration Division announced its eagerly awaited Discovery selections to follow Pathfinder and Near-Earth Asteroid Rendezvous (NEAR). Getting the nod for development was the Lunar Prospector, intended to map the chemical composition of the lunar surface and its magnetic and gravity fields. Three other missions, targeted on a cometary coma, Venus, and solar particle streams, were selected for further definition studies for possible later development.

On March 1-3, the Space Studies Board held its 115th meeting at the National Academy of Sciences in Washington, D.C. Members had an opportunity to hear from Drs. Jack Fellows and Sarah Horrigan of the Office of Management and Budget, and from Mr. David Moore of the Congressional Budget Office, on the overall funding outlook for NASA. Drs. France Cordova, Harry Holloway, Charles Kennel, and Wesley Huntress provided detailed insight into status and planning within the agency's science programs. Mr. Alan Ladwig, NASA associate administrator for policy and plans, discussed strategic planning at the agency level and the role of outside organizations in this planning. Reports were heard from Drs. Val Fitch and Michael A'Hearn on the Gravity Probe B and BMDO 4-meter telescope task group activities, respectively, and the Board was briefed by Mr. Robert Winokur, NOAA assistant administrator for the National Environmental Satellite Data and Information Service (NESDIS), on progress in setting up the Integrated Program Office for the polar-orbiting environmental satellite system. European Space Science Committee (ESSC) Chair François Becker briefed the Board on the evolving organization of that committee; this discussion led to broad agreement that the Board and the ESSC should undertake a joint "lessons-learned" study on international collaborations in space research.

During the meeting, the Board approved its 1995 operating plan, a new task on data and sample archiving for the Committee on Microgravity Research, and the lessons-learned report on *Clementine* of the Committee on Planetary and Lunar Exploration. Informal agreement was reached on preliminary plans for a Board project on NASA's Research and Analysis (R&A) programs.

NASA Chief Scientist Cordova formally requested the Board's perspectives on the role of science and scientists at the NASA field centers as an input to internal NASA management discussions. Board members had several opportunities to discuss the topic during the meeting and, recognizing that a full treatment of this complex issue would have to wait until issuance of the FOSS report near the end of the year, prepared a letter report that was subsequently delivered to Dr. Cordova on March 29 (Section 4.1).

During the second quarter, the three big issues were NASA's Zero Base Review (ZBR), the budget, and Shuttle-Mir. Begun during 1994, the ZBR gathered up and integrated the results of a number of agency management studies that spanned space shuttle operations, the field center system, and NASA's labor force. Key inputs were Administration and OMB guidance and REGO-2. A major driver in the exercise was identifying the extra \$5 billion that the Administration had decided in January to cut from the agency's 5-year budget runout to finance a tax cut.

Building on a legacy of center “roles and missions” studies, including notably the analysis carried out a few years ago by former Deputy Administrator J.R. Thompson, the ZBR recommended that each center have a “primary mission” aligned with one of the strategic enterprises in NASA’s strategic plan. Other major themes in the ZBR were improved financial controls, including “industrial accounting,” to allocate all direct and indirect costs for all programs, and privatization wherever possible. Shuttle operations were identified as especially suitable for privatizing. An operating guideline of the ZBR was to eliminate to the extent possible all non-civil servant (or non-JPL employee, at that center) in-house research work. Research programs not aligned with their parent center’s primary mission would be considered for spin-off into a privatized “institute,” which would compete with other outside community scientists for funding. At the same time, the ZBR provided that NASA Headquarters staffing would continue its downward glide to an overall 50% reduction, and total agency employment was projected to decline from 21,060 in April 1995 (down from 24,030 in January 1993) to a planned 17,500 in 2000. The bulk of ZBR cuts would be taken from “infrastructure,” but no field centers were to be closed. The plan strove to protect “program,” in contrast to infrastructure, and in fact no science programs were to be terminated under the NASA austerity plan.

In releasing the ZBR results on May 19, Administrator Goldin expressed satisfaction with the results of the grueling effort: “I’m pleased with what I’ve seen so far. . . . We’ve found ways to streamline operations, reduce overlap, and significantly cut costs without cutting our world-class space and aeronautics programs. We have much hard work before us, but I believe a stronger and more efficient NASA will emerge.” Unfortunately, the day before the ZBR’s public release, the U.S. House of Representatives passed a budget resolution that once again ratcheted down NASA’s budget limbo-bar as part of the new Congress’ drive for a balanced budget in 2002. In particular, the ZBR plan’s \$13 billion agency budget for 2000 would now be \$11.7 billion, according to the House. Because the projections were not corrected for inflation, inflationary erosion would compound the decline implied by these reductions from the FY95 figure of \$14.3 billion. Mr. Goldin promised to fight the new round of cuts. As the House budget resolution went to conference with its Senate counterpart in mid-June, attention began to shift to the appropriations process, in which the true implications of the budget resolutions would be hammered out. The 1994 Congressional Budget Office report *Reinventing NASA* took on fresh interest.

In contrast to the turmoil on the ground, the elegance and delicate grace of spaceflight thrilled television viewers as the Atlantis and Mir docked for the first time on June 29. Almost 20 years after the first U.S.-Russian orbital link-up in the Apollo-Soyuz program, astronauts and cosmonauts greeted each other again through a docking adapter far above Earth. Images of the historic shuttle-Mir coupling, in which crew were rotated out of a Russian space station by a U.S. shuttle, seemed to lend to the space station program a solidity that repeated congressional reaffirmations have not.

On June 8 and 9, the Board held its 116th meeting in Washington, D.C. Regular Board business was compressed into the first day of the meeting, so that the second could be devoted to a joint meeting with the steering group of the FOSS project. First, NASA Chief Scientist Cordova reviewed current planning issues in the space sciences, with a special emphasis on the new “science institute” concept that had emerged as part of the ZBR. Dr. Cordova presented the Board with several specific questions for discussion and requested a formal response from the Board. The Board collected its conclusions in a letter forwarded to Dr. Cordova on August 11 (Section 4.5). The afternoon session focused on long-range planning issues specific to NASA’s Space Physics and Astrophysics Divisions; a review of ESSC status and plans, including a proposed joint SSB-ESSC study, was presented by ESSC Chair Becker. Also in attendance were ESSC Deputy Chair Herbert Schnopper and Scientific Assistant Jean-Claude Worms. The second day of the meeting began with a presentation to the Board of the status and preliminary findings of the FOSS project by steering group Chair John Armstrong and task group Chairs Daniel Fink, Roland Schmitt, and Anthony England. After a break, Associate Administrators Huntress and Kennel and Deputy Associate Administrator Arnauld Nicogossian joined the Board for a discussion of the ZBR, including its philosophy, findings, and implications for NASA’s science offices. During the course of the meeting, committee reports from the Board’s Committees on Microgravity Research, Earth Studies, Space Biology and Medicine, and Planetary and Lunar Exploration were also heard and discussed. Planning was reviewed for Board tasks on NASA R&A and on research mission effectiveness and risk. A draft letter report by the Committee on Space Biology and Medicine was also approved (Section 4.4).

The third quarter of 1995 started out with a bang on July 10 when NASA’s House appropriations subcommittee recommended a radical restructuring of NASA and its priorities. Only 5% below the President’s FY96 request

overall, the subcommittee's package provided for cancellation of Cassini and Gravity Probe B and closure of Goddard Space Flight Center, Marshall Space Flight Center, and Langley Research Center. At the same time, the shuttle, space station, Advanced X-ray Astrophysics Facility, and Earth Observing System (EOS) were untouched. But barely a week later, the full committee reversed the most radical provisions of the initial package, restoring all of the above cuts, but reducing EOS by \$333 million. The accompanying committee report expressed satisfaction with the ability of the plan resulting from NASA's Zero Base Review to achieve the long-range reductions that had been foreseen at the end of 1994 but directed the agency to complete by March 31, 1996, a further study that would accommodate a new and larger retrenchment. The committee report suggested that NASA might have to consider closing centers or eliminating major programs after all, contrary to guidelines NASA had set for itself for the initial Zero Base Review. The bill passed the House on July 31, and attention shifted to the Senate.

The corresponding bill reported out of the Senate appropriations committee on September 13 was more favorable to space science in many areas. For example, some reduced funding was included for the House-zeroed Space Infrared Telescope Facility, greatly improving its prospects for a new start in the conferenced legislation, and the House cut in EOS was reduced from 22% of the President's request to essentially nil. As the September 30 fiscal year end approached, the final outcome of space funding for FY96 was subsumed into the general budget stand-off and 6-week continuing resolution negotiated between the Congress and the White House to avert a threatened government-wide shutdown. While the rapid growth of space science spending of recent years was now clearly at an end, researchers would have much to be pleased about if a compromise between House and Senate budgets could survive subsequent negotiations and become law.

The full Space Studies Board did not meet during the third quarter, but its executive committee (XCOM) met at Woods Hole, Massachusetts, on August 8-10. The first day of the meeting was held in joint session with the FOSS Steering Group. After discussion of the suggestions for the group's draft report that were obtained from a review by the full Board during July, and consideration of proposed revisions, the XCOM approved the report for submission to NRC institutional review. The XCOM spent the next day and a half reviewing activities of individual Board committees and acted on a number of proposed new membership nominations and projects. It approved new projects on small satellites (Committee on Earth Studies), on the small and mid-sized explorer (SMEX/MIDEX) mission lines (Committee on Solar and Space Physics), and on near-Earth asteroids, trans-Neptune solar system science, and the role of surface mobility in planetary space probes (three studies for the Committee on Planetary and Lunar Exploration). In order to improve communication between the Board and the Committee on Astronomy and Astrophysics, the XCOM approved creation of a committee co-chairmanship to be occupied by an astronomer member of the Board. The XCOM also gave concurrence to a Committee on Astronomy and Astrophysics study on National Science Foundation infrastructure funding and did additional planning for the Board's studies on NASA R&A, research mission effectiveness and risk, and international space science collaborations. A draft study plan addressing new NASA space astronomy missions was forwarded to the Committee on Astronomy and Astrophysics for consideration.

During the fall, NASA's "science institutes framing committee" was completing its work and beginning to brief outside advisory groups and the scientific community. NASA executives explained how establishment of the new institutes would improve the overall quality of NASA's science programs, and began to sift through the many practical, statutory, and equity issues involved in creating the first institutes at the Johnson Space Center and the Ames Research Center. The U.S. Microgravity Laboratory-2 shuttle Spacelab mission, which ended after 16 days on-orbit in early November, was reported to have produced excellent results.

The Board held its 117th and final meeting of 1995 on November 28-30 at the NRC's Beckman Center in Irvine, California. The main topics were a series of videoconferences with government officials and project status reports and approvals for the Board's committees. NASA Chief Scientist Cordova presented a preview of the agency's developing responses to the Board's FOSS report, *Managing the Space Sciences*. It became apparent that the term "concurrence" was being understood differently than the report had intended, and so the Board drafted a short letter of clarification to the chief scientist (Section 4.6). Associate Administrators Huntress, Holloway, and Kennel presented the status of science programs in their offices. Later in the meeting, Deputy Associate Administrator Alphonso Diaz summarized the recommendations of his institute framing committee, recapitulating and elaborating this leitmotif that had run through all the science office briefings. In another videoconference, Dr. Horrigan, Office of Management and Budget program examiner, explained the intent and requirements of the Government Performance and Results Act of 1993. The Board was pleased to have visits from Committee on Astronomy and Astrophysics Co-Chair Marc Davis and ESSC Deputy Chair Schnopper.

In addition to routine status reports from its discipline committees, the Board considered two completed reports and a new project. A Committee on Planetary and Lunar Exploration report on the Mars Surveyor program was remanded to the committee for expansion in several areas, and a Committee on Microgravity Research report on data and sample archiving and retrieval was approved subject to revision. A new NASA-requested project on sample return from Mars or near-Earth objects was approved. A splinter breakfast meeting was held to sharpen the focus of the proposed new project on research mission effectiveness and risk to be conducted jointly with the NRC's Aeronautics and Space Engineering Board. Space Studies Board members were pleased to welcome ASEP member Donald Kutyna and ASEP Director JoAnn Clayton for the discussion.

Tuesday, December 19, symbolized in a single non-event both elation and frustration in the last quarter of 1995. There was to have been a NASA press conference on early results of the Galileo entry probe, which had hurtled dramatically into the jovian atmosphere two weeks before. But NASA was closed, shuttered again for the second time in as many months, a victim of the continuing conflict over appropriations between the Congress and the White House. While enough bills had been signed to exempt 600,000 federal workers from the second lockout, the VA-HUD-IA appropriations bill was one of several that the President vetoed that week, closing the space agency. Ironically, the vetoed VA-HUD-IA bill was itself the result of an unusual process whereby the House-Senate conference bill was rejected by the House briefly before being passed and then forwarded for executive rejection.

Also during the final days of 1995, a new chapter in the space station odyssey seemed about to unfold. Based on a December 26 report in the *Washington Post* (with NASA still largely furloughed, official information was scarce), the Russians proposed to continue their current Mir station in service as the "hub" of the new International Space Station. NASA Administrator Goldin reportedly countered with the suggestion that if the Russians would be able to deliver elements that are, quoting from the *Post*, "critical to the completion of the new space station," then "instead of building new laboratory modules for the new facility . . . the Russians could decide to shift two of the most modern laboratory segments from Mir when the time comes." But any reconsideration of the fundamental structure and composition of the station, especially in the prevailing fiscal climate and without a signed NASA appropriation for FY96, presented the possibility that the long-standing debate over the program could be reopened.

Solar researchers were delighted when the Solar and Heliospheric Observatory (SOHO) was successfully launched on December 2 and began making its way to its station at the Earth-Sun L1 point. The X-ray Timing Explorer was successfully launched, after a number of launcher-related delays, just a few days before the end of the year.

The last days of 1995 found the nation's space program without an appropriation, held hostage in broader disputes that included fundamental disagreements about the Environmental Protection Agency, also funded in the VA-HUD-IA bill. While the general outlook for NASA was more uncertain than directly threatened, the continuing stalemate and furlough of agency personnel impacted planning, progress, and morale across the space program. Even with its disabled high-rate antenna, Galileo's successful entry probe deployment after a six-year cruise was a bright beacon over a turbulent and anxious space research landscape.

Membership of the Space Studies Board

Claude R. Canizares,§ Massachusetts Institute of Technology (chair)

John A. Armstrong, IBM Corporation (retired)

James P. Bagian, Environmental Protection Agency

Daniel N. Baker, University of Colorado

Lawrence Bogorad,§ Harvard University

Donald E. Brownlee, University of Washington

Joseph A. Burns,* Cornell University

John J. Donegan, John Donegan Associates, Inc.

Anthony W. England, University of Michigan

Daniel J. Fink, D.J. Fink Associates, Inc.

Martin E. Glicksman, Rensselaer Polytechnic Institute

Ronald Greeley, Arizona State University

William Green, New York, New York

Harold J. Guy,* University of California at San Diego

Noel W. Hinnners,§ Lockheed Martin Astronautics
 Janet G. Luhmann, University of California at Berkeley
 John H. McElroy, University of Texas at Arlington
 Roberta Balstad Miller,§ Consortium for International Earth Science Information Network
 Berrien Moore III, University of New Hampshire
 Mary Jane Osborn, University of Connecticut Health Center
 Simon Ostrach,§ Case Western Reserve University
 Carlé M. Pieters,§ Brown University
 Judith Pipher,* University of Rochester
 Marcia J. Rieke, University of Arizona
 Roland W. Schmitt, Rensselaer Polytechnic Institute (retired)
 John A. Simpson, University of Chicago
 Arthur B.C. Walker, Jr.,* Stanford University
 Robert E. Williams, Space Telescope Science Institute

Louis J. Lanzerotti, AT&T Bell Laboratories (ex officio, U.S. representative and vice president of COSPAR)
 François Becker, Ecole Nationale Supérieure de Physique (liaison from the European Space Science Committee)
 Marvin A. Geller, State University of New York at Stony Brook (ex officio, chair of the Committee on Solar-Terrestrial Research)
 Jack L. Kerrebrock, Massachusetts Institute of Technology (ex officio, chair of the Aeronautics and Space Engineering Board)
 Vincent Vitto, Massachusetts Institute of Technology (ex officio, chair of the Naval Studies Board Space Panel)

Marc S. Allen, Director
 Betty C. Guyot, Administrative Officer
 Anne K. Simmons, Administrative Assistant

*term ended during 1995

§member of the Executive Committee

COMMITTEE ON INTERNATIONAL PROGRAMS

The Board's international program advanced importantly during late 1995. The incoming chair of the Board's Committee on International Programs (CIP), Dr. Berrien Moore, participated in the September 14-15 meeting of the European Space Science Committee (ESSC) held at the Rutherford-Appleton Laboratory in Oxfordshire, England. A significant item on the meeting agenda was initiation of the long-planned joint study of U.S.-Europe collaboration in space science. Both the Board and ESSC brought to the meeting lists of candidate missions for study. The two lists were discussed, and general agreement was reached that the study should consider only missions that have already flown and produced significant scientific return. There was also general agreement that an additional category of programs should be added to those identified by the Board—international data collection networks (e.g., that for AVHRR). It was also decided that the final list of missions would be selected by the joint study panel, once formed, from the Board-ESSC list. Plans were laid for the first meeting of the joint study to take place in conjunction with the next ESSC meeting, in April 1996, at a location in Europe. The second and final meeting of the joint study panel would take place in the United States, perhaps at Woods Hole, Massachusetts, in September 1996.

A preliminary nominations list for the CIP was prepared and circulated for comment and Board approval.

JOINT COMMITTEE ON TECHNOLOGY FOR SPACE SCIENCE AND APPLICATIONS

The Joint Committee on Technology for Space Science and Applications, a cooperative activity of the Space Studies Board and the NRC's Aeronautics and Space Engineering Board, operated during 1994 as the Future of Space Science (FOSS) project's Task Group on Technology. The FOSS project and the activities of its task groups are described below in a separate subsection.

COMMITTEE ON ASTRONOMY AND ASTROPHYSICS

The Committee on Astronomy and Astrophysics (CAA) met in Washington, D.C., on April 21-22. A major agenda item was the formal release of the report of the Panel on Ground-Based Optical and Infrared Astronomy (OIR Panel), chaired by CAA member Richard McCray, entitled *A Strategy for Ground-Based and Infrared Astronomy* (Section 3.1). McCray reported on briefings to staff members in Congress and on a meeting with NSF Director Neal Lane. The Council of Directors of Independent Astronomical Observatories drafted a letter supporting the OIR Panel's recommendations. Dr. Hugh Van Horn, director of the Astronomical Sciences Division at NSF, described steps being taken by the National Optical Astronomy Observatories in response to the OIR Panel's report. NASA Astrophysics Division Director Daniel Weedman briefed the committee, noting that if Congress were to fund SOFIA and SIRTf in the FY96 budget, all the priorities of the 1991 Astronomy and Astrophysics Survey Committee report (the Bahcall report) would have been addressed, except for the Astrometric Interferometry Mission. By the end of the decade NASA expects to launch AXAF, FUSE, SWAS, SMIM, and WIRE and to be involved in the international missions ISO, FIRST, Astro-E, and Integral. These missions would empty a 30-year queue, and so there was a need to begin planning for new starts after 2000. Drs. Weedman and Van Horn agreed that an astronomy and astrophysics survey, with a prioritized list of mission concepts and a strong community consensus behind it, would be useful for the FY00 budget planning cycle. The committee decided to consider when to begin the next survey at its fall meeting after the FY96 budget was settled.

Dr. Holland Ford discussed the Polar Stratospheric Telescope and mentioned the Space Studies Board task group assessing the 4-meter orbital telescope. Board Director Marc Allen described the Board's Future of Space Science project. The committee developed written comments for this project and formed an ad hoc subcommittee to provide additional input, chaired by Dr. Marc Davis, with Drs. Jonathan Grindlay and John Huchra. Dr. Allen suggested that the committee consider generating a science strategy for NASA astronomy and astrophysics and requested input from the committee on the Board's project to examine international space collaborations.

Space Telescope Science Institute Director Robert Williams gave a talk on the Hubble Space Telescope's scientific discoveries since the repair mission. The Russian perspective on international space collaboration was provided by Dr. Rashid Sunya'ev, who reported on Russian space missions. He expressed concern about the "brain drain" to the West and the loss of interest in science by young people. In a joint session of the committee with the NRC's Board on Physics and Astronomy, Mr. Steven Merrill, director of the NRC's Office of Congressional and Government Affairs, discussed current political activity in Congress and what he foresaw as a new era for science.

The committee met again on September 15-16 in Washington, D.C. Senior NASA and NSF management briefed the committee on the rapidly changing budget situation at NASA and the NSF and its impact on astronomy and astrophysics. The president and board director of the Associated Universities for Research in Astronomy (AURA), Inc., Drs. Goetz Oertel and Bruce Margon, discussed AURA's views on the strategy prepared by the OIR panel. In connection with the panel's recommendations and a proposed follow-on study on astronomy infrastructure, the directors of federal astronomy centers—the National Optical Astronomy Observatories, the National Radio Astronomy Observatory, the National Astronomy and Ionosphere Center, and the Space Telescope Science Institute—described their plans and needs for the near future.

The committee considered several possible new undertakings: (1) an examination of the implementation, primarily by the NSF, of recommendations on astronomy grants and infrastructure, the highest priority for ground-based astronomy in the 1991 survey report, (2) a review of concepts for future NASA space astronomy and astrophysics missions and preparation of a science strategy, (3) an assessment of the scientific value of having the Space Infrared Telescope Facility (SIRTf) in orbit at the same time as the Hubble Space Telescope and the Advanced X-ray Astrophysics Facility (AXAF), and (4) timing for initiation of the next decadal astronomy and astrophysics survey. It was decided to pursue the requested space astronomy strategy and mission assessment and to draft a short report on optimizing the operational availability of SIRTf in concert with the other Great Observatories.

CAA Membership

Marc Davis, University of California at Berkeley (co-chair)

Marcia J. Rieke, University of Arizona (co-chair)

Leo Blitz, University of Maryland

Arthur F. Davidsen, Johns Hopkins University

Holland C. Ford,* Space Telescope Science Institute
 Wendy Freedman, Carnegie Observatories
 Jonathan E. Grindlay, Harvard-Smithsonian Center for Astrophysics
 Doyal A. Harper,* Yerkes Observatory
 John P. Huchra, Harvard-Smithsonian Center for Astrophysics
 Kenneth I. Kellermann, National Radio Astronomy Observatory
 Richard A. McCray, University of Colorado at Boulder
 Jeremiah P. Ostriker,* Princeton University
 Robert Rosner, University of Chicago
 Bernard Sadoulet, University of California at Berkeley

Robert L. Riemer, Study Director
 Anne K. Simmons, Administrative Assistant

*term ended during 1995

COMMITTEE ON EARTH STUDIES

The Committee on Earth Studies (CES) held a workshop on synthetic aperture radar (SAR) on January 9-11 at the Beckman Center. The purpose of the workshop was for the committee to hear briefings on the use and impact of SAR technology on specific fields of Earth sciences research, and to develop advice for NASA regarding future SAR flights. NASA and NOAA representatives were in attendance, as were speakers from academia and industry.

The meeting's general session began with a background discussion by Dr. Miriam Baltuk, SAR program scientist at NASA Headquarters. She noted that NASA was looking for advice on the next step in its SAR program and was specifically seeking a recommendation on flying a third shuttle SAR mission. Mr. Robert Winokur, NOAA assistant administrator for NESDIS, gave a discussion on operational requirements compiled by his interagency panel. This discussion was followed by reports from a number of science panels on the topics of oceanography, solid Earth, terrestrial ecology, ice sheets and glaciers, hydrology, and technology. The strongest case for a third flight was made by the solid Earth panel, which concluded that a third mission, using a modified SAR, could provide an important advance in the knowledge of global topography.

After the science presentations, the committee decided on a list of 13 themes to organize its advice to Dr. Charles Kennel, the associate administrator for Mission to Planet Earth (MTPE). The workshop participants broke into discipline splinter groups to develop input on these themes and reported these back to the full committee before adjourning. These inputs were used by Chair McElroy to draft a brief report containing recommendations (Section 4.2). The committee planned to develop a broader and more detailed assessment of SAR programs and technologies during the coming year.

The committee met on May 1-3 in Washington, D.C. Chair John McElroy opened the meeting and updated the committee on the status of the *Earth Observations from Space* report (Section 3.4), which was under review and revision. Time was later spent ironing out details of the report's presentation. Associate Administrator Kennel addressed the committee on a variety of issues. Of significant interest was an update on the state of NASA restructuring and its future effects on scientific research. He also touched on the status of the Earth Observing System (EOS) program in this context. Other guests at the meeting included Dr. Dixon Butler, MTPE's director of operations, data, and information systems, and Mr. Winokur of NOAA. Dr. Butler briefed the committee on the status and future of the EOS Data and Information System (EOSDIS). Mr. Winokur described the details of NOAA satellite programs including the Geostationary Operational Environmental Satellite (GOES) and Polar-Orbiting Operational Environmental Satellite (POES) systems, the planned National Polar-Orbiting Environmental Satellite System (NPOESS), the Landsat program, and the Ocean Remote Sensing Program, and also gave an overview of the NASA-NOAA realignment survey. This latter survey was initiated to identify possible cost savings that could be achieved through closer NOAA and NASA interaction. As a continuation of the committee's ongoing evaluation of SAR, presentations were given by representatives of various agencies. The view of the international community was presented by representatives from Germany, Japan, Italy, and the European Space Agency. Further input was also received from NASA, NOAA, and JPL representatives.

A prepublication copy of *Earth Observations from Space* was made available to participants in the July 19-28 workshop on the U.S. Global Change Research Program (USGCRP) and NASA's MTPE/EOS program.

The committee met August 21-23 in Washington, D.C., to hear additional briefings on SAR programs and on plans for small satellite programs at NASA and NOAA. In his opening comments Chair McElroy summed up events at the July USGCRP workshop, which was held at La Jolla by the NRC's Board on Sustainable Development. There was discussion about the La Jolla workshop recommendations on the elimination or decentralization of the EOS data archive systems. A status briefing on NASA's MTPE was given by MTPE Associate Administrator Kennel, followed by Mr. Ernest Paylor of NASA, who discussed the future of SAR. Committee member Richard Moore provided a technical tutorial on the principles of SAR.

The general session the following morning was opened with briefings on NASA's New Millennium program and the OSAT technology program by Dr. Kane Casani of JPL, Mr. Sam Venneri of NASA's Office of Space Access and Technology (OSAT), and Mr. Gran Paules of MTPE. The committee understood the New Millennium program to be primarily a new way of managing mission development and execution for the observing sciences. Mr. Winokur of NOAA gave a briefing covering recent studies initiated by NOAA on SAR and small satellites, and other NOAA status issues such as ongoing budget negotiations. The committee then went into executive session to discuss the SAR and small satellite studies. The meeting adjourned at noon on the third day, with Dr. McElroy taking responsibility for revising the SAR report. Attendance by General Accounting Office staff during both days of open meetings was noted by the committee.

On November 15, the committee met in Washington, D.C., for the first of three planned meetings to analyze the ability of small satellites to satisfy "core" observational needs of NASA and NOAA, including weather and climate monitoring and prediction programs. Budgetary pressures, the desire within the scientific community for shorter mission time lines, gaps and opportunities in planned MTPE programs, and calls for more rapid technology infusion focused renewed attention on the possible use of small satellites in the EOS program. Similarly, interest in small satellites was heightened by NOAA and DoD (USAF) plans to forego planned block upgrades and instead design a common spacecraft system (the NPOESS) to fulfill the functions currently performed by POES and the Defense Meteorological Satellite Program (DMSP). Chair McElroy reviewed the background of the small satellite study and the time table for completion of the SAR report. The former study will build on an NRC study of technology for small satellites and the recent La Jolla summer workshop to review the status and scientific foundations of EOS and the USGCRP. It will also take into account the widely publicized DoD-NASA Clementine small satellite mission, NASA's initiation of the Lewis and Clark demonstration missions, and the New Millennium technology development program.

NASA participation in the meeting was precluded by the government-ordered shutdown of "non-essential" services, but committee members were given copies of a NASA presentation on MTPE status prepared by Mr. William Townsend. Mr. Winokur, director of NOAA/NESDIS, reviewed the status of NOAA's satellite programs and the NPOESS. Dr. Fuk Li, of the JPL, reviewed JPL efforts to develop a lightweight SAR and its possible applications. After Dr. Li's presentation, there was discussion about the utility of a system that had limited ground coverage and long repeat times. According to Dr. Li, the New Millennium program will demonstrate a number of advanced spacecraft, sensor, and processing technologies that might find future application in the EOS and NPOESS. Dr. Li spoke in particular about the advantages of developing spacecraft with "intelligent" flight systems and greater autonomy.

Dr. Walter Scott, of EarthWatch, began his presentation with an introduction to EarthWatch and a description of its first planned satellite mission, EarlyBird, scheduled for launch in 1996. EarlyBird's 3-meter resolution corresponds to typical airborne imager maps. QuickBird was described as EarthWatch's next planned system, to feature a 1-meter panchromatic sensor.

Mr. David Johnson, of CTA, presented a review of the Clark spacecraft and its role in increasing the capability and reducing the cost of small spacecraft. A spirited discussion followed on why Landsat costs are so high and whether the government should develop and launch its own remote sensing satellites. Next, Dr. Frank Martin, of Lockheed Martin, talked on three of the questions participants had been asked to consider prior to their attendance at the meeting: (1) Have new spacecraft technologies emerged that can enhance payload mass, power, and volume fractions, or reduce overall systems cost? (2) As a result of technological advances, are there ways of aggregating or parsing sensor systems (including satellite constellations) that can offer advantages in the cost or effectiveness of Earth observations? (3) If such ways exist, what are their implications for the future of MTPE, POES/DMSP, and GOES in terms of both spacecraft and ground system configurations? Dr. Bruce Marcus, of TRW, presented results of a TRW study of alternative NPOESS space segment architectures. This study was an outgrowth of preliminary work from the NRC July 1995 La Jolla summer workshop on MTPE/EOS and USGCRP. Mr. Robert Barry, of Ball

Aerospace and Technology Corporation, described several of Ball's small satellite ventures, altimetry, and Ball's Geosat Follow-On (GFO), which is being designed to be a long-life operational small satellite for the Navy. Mr. Barry noted that the government contract with Ball for GFO gives great responsibility to the contractor for managing the project and strong incentives for successful on-orbit performance.

CES Membership

John H. McElroy, University of Texas at Arlington (chair)

William D. Bonner, University of Colorado

George H. Born,* University of Colorado

John V. Evans, COMSAT Laboratories

Inez Y. Fung, University of Victoria

Elaine R. Hansen, University of Colorado at Boulder

Roy L. Jenne, National Center for Atmospheric Research

Pamela E. Mack, Clemson University

Richard K. Moore, University of Kansas

Stanley Morain, University of New Mexico

Peter M.P. Norris, Santa Barbara Research

Clark Wilson,* University of Texas at Austin

Arthur Charo, Study Director

Richard C. Hart, former Study Director

Carmela J. Chamberlain, Administrative Assistant

*term ended during 1995

COMMITTEE ON HUMAN EXPLORATION

The Committee on Human Exploration (CHEX) was inactive during 1995. Preliminary discussions were held with officials of NASA's Office of Life and Microgravity Sciences and Applications about restarting the committee for the purpose of completing and issuing a previously tabled report on science management within human spaceflight programs.

COMMITTEE ON MICROGRAVITY RESEARCH

The Committee on Microgravity Research (CMGR) met on February 8-9 in Washington, D.C., to discuss its next study. The director of NASA's Microgravity Science and Applications Division (MSAD), Mr. Robert Rhome, presented a detailed overview of the current microgravity program to the committee. Drs. Rose Scripa and Robert Snyder, both of MSAD, were present to answer questions. The overview was followed by a presentation from Mr. Rhome, at the previous request of the committee, on potential studies that would be particularly useful to MSAD. After some discussion, the committee decided to take up a study evaluating the current MSAD strategy for archiving flight data and samples. The final topic of discussion before adjournment was the committee's report *Microgravity Research Opportunities for the 1990s* (Section 3.2). The report was in press at the time of the meeting, and advance copies had been made available to MSAD for planning purposes. Mr. Rhome presented the committee with a series of questions seeking clarification of the report's contents. These questions arose from changes that had taken place in the agency since the report was initiated, such as the drive to consolidate division activities. There was some discussion of the questions, and the meeting was concluded with the decision that the committee would develop written responses to the questions to accompany publication of the report (Section 4.3).

The committee held a second meeting on May 22-23 in Washington, D.C., to begin work on the data and sample archiving study. Invited speakers made detailed presentations on the development and management of the Hubble Space Telescope archives, the Planetary Data System, and USGCRP. In addition to learning about these observational data archives, the committee was briefed on a large, materials science data archive by Dr. Gil Kaufman of the Aluminum Association. Board staff member Richard Hart gave a talk on the findings of previous data management studies by the Board's defunct Committee on Data Management and Computation. After the

committee had had an opportunity to become familiar with the issues, problems, and “lessons learned” associated with a variety of archives, NASA speakers briefed the committee on the current archiving strategy employed by MSAD. MSAD’s Mr. Gary Martin gave a talk outlining the overall philosophy and design of the plan for archiving microgravity experiment data and samples. MSAD Chief Scientist Roger Crouch was also present to answer questions. The archives for MSAD flight experiments are distributed between two NASA field centers, and the committee heard briefings from Drs. Howard Ross and Laura Maynard of the Lewis Research Center (LeRC), and from Drs. Robert Snyder and Charles Baugher of the Marshall Space Flight Center (MSFC), about the content and management of their respective archives. The committee also saw demonstrations of access to on-line directories for these archives through the Internet. At the time of the meeting, only one mission, the 1992 flight of USML-1, had followed MSAD’s full archival procedure, and it was not clear how much of that data was yet accessible. MSAD described its approach as “minimalist,” and Dr. Crouch expressed his concern that a more extensive approach might force MSAD into a “one size fits all” archival strategy in the future. Prior to adjournment, committee members agreed that before the next meeting they would attempt to access the archives on their own and report back on their findings.

The committee met a third time on August 26-29 at Woods Hole, Massachusetts, to continue the assessment of MSAD’s data and sample archiving strategy. The meeting began with opening remarks by Chair Martin Glicksman, followed by a roundtable discussion of the experiences of each committee member in trying to access information on the NASA microgravity archives through the Internet. Mr. Rhome and Mr. Martin of MSAD were present for this discussion. The following morning, Drs. Judy Allton and Michael Zolensky of the Lunar and Planetary Institute, NASA Johnson Space Center, described in detail how physical samples, ranging from lunar specimens to interstellar dust particles, are carefully stored, catalogued, and accessed for future use. These procedures included keeping the research community informed of available resources and determining how requests for samples would be resolved. Dr. Snyder of MSFC gave a presentation to the committee on the archiving of samples at that center. Most of MSFC’s experience in this area is with Skylab experiments, inasmuch as virtually none of the samples collected from Spacelab have been returned to NASA and there are few restrictions on how principal investigators must manage their samples. Mr. Rhome gave a presentation on the current status of MSAD, including NASA’s plans to further reduce the size of MSAD’s headquarters staff. The remainder of the day was devoted to a demonstration of actual, digitally archived flight data (acquired from LeRC) and discussion of the archiving report. By the end of the afternoon the committee had come to agreement on many of the major recommendations that would be presented in the report. Discussion continued the following morning, and the meeting adjourned in mid-afternoon with an agreement that a first draft of the report would be assembled by the committee chair and study director.

The committee met for its final gathering of 1995 on November 8-9 at the Beckman Center to finalize the data and sample archiving report. The meeting opened with discussion on the recent Japan Space Utilization meeting in Tokyo, at which staff member Sandra Graham had presented a talk on the committee’s *Opportunities* report, and on the U.S.-Russian microgravity conference in Washington, D.C., where member Robert Bayuzick had made a similar presentation. Committee members’ comments on a draft of the archiving report prepared after the previous meeting had been integrated into the report prior to this meeting. The committee spent most the morning of the first day discussing unresolved issues in the report and spent the remainder of the day rewriting the report. The committee as a whole devoted the second day of the meeting to reviewing, discussing, and editing this new version, line by line. These changes were integrated at the completion of the meeting so that the report would be ready to enter NRC review.

CMGR Membership

Martin E. Glicksman, Rensselaer Polytechnic Institute (chair)
Robert A. Altenkirch, Washington State University
Rosalia N. Andrews,* University of Alabama at Birmingham
Robert J. Bayuzick, Vanderbilt University
Gretchen Darlington, Texas Children’s Hospital
Howard M. Einspahr, Bristol Myers Squibb Company
J. Gilbert Kaufman, Jr., The Aluminum Association
L. Gary Leal, University of California at Santa Barbara

Ronald E. Loehman, Sandia National Laboratories
Margaret J. Lyell, West Virginia University
Morton B. Panish, AT&T Bell Laboratories (retired)
Ronald F. Probst, Massachusetts Institute of Technology
John D. Reppy, Cornell University
Julian Szekely, Massachusetts Institute of Technology (deceased)
Forman A. Williams, University of California at San Diego

Sandra J. Graham, Study Director
Victoria Friedensen, Administrative Assistant

*term ended during 1995

COMMITTEE ON PLANETARY AND LUNAR EXPLORATION

The Committee on Planetary and Lunar Exploration (COMPLEX) met at the Beckman Center on February 8-10 to begin work on a study of NASA's Mars exploration plans. Chair Joseph Burns updated the committee on the status of its reports, including *An Integrated Strategy for the Planetary Sciences: 1995-2010* (released in late 1994), *The Role of Small Missions in Planetary and Lunar Exploration* (Section 3.7), and *Lessons Learned from Clementine*. The bulk of the meeting was devoted to briefings on current understanding about Mars and to the geophysical, geological, atmospheric, and exobiologic goals of Mars exploration. Dr. Dan McCleese, JPL, briefed the committee on plans for the Mars Surveyor program and emphasized the tight constraints facing the program. Dr. Lewis Franklin, of Stanford University, explained how some of these constraints might be overcome if a variety of Russian launch vehicles, including refurbished ICBMs, were used. Following discussions in executive session, it was decided that the committee would produce a short report on Mars science, and writing tasks for an initial draft were assigned. Dr. Jurgen Rahe, director of NASA's Solar System Exploration Division, briefed the committee via videoconference on his division's recent review of R&A programs, the selection of Discovery mission(s), and the Astronomical Search for Extra-Solar Planetary Systems program. He also gave NASA's preliminary response to the committee's *Integrated Strategy*. The meeting concluded with Dr. Wesley Huntress, associate administrator for space science, briefing the committee via videoconference on the details of NASA's FY96 budget, the long-term future of the planetary exploration program, and the likely role of international cooperation.

The committee met again in Woods Hole on June 12-16 to continue work on its study of NASA's Mars exploration plans. Chair Burns reported that the committee's report *The Role of Small Missions in Planetary and Lunar Exploration* had been approved by the NRC for publication (Section 3.7) and that advance copies had been delivered to Drs. Huntress and Rahe at NASA Headquarters. Another report, *Lessons Learned from Clementine*, had been approved by the Board and was being revised for external review. In addition to finalizing the text of the Mars report, the committee devoted time to planning future activities. Topics under consideration included exploration of the trans-Neptune solar system, the role of mobility in planetary exploration, exploration of near-Earth asteroids, and an assessment of ground- and space-based planetary astronomy facilities. The remainder of the meeting was spent on a presentation on the role of NASA's Solar System Exploration Subcommittee, the ongoing Zero Base Review, the status of planetary missions, NASA's Discovery program, exobiology, and terrestrial impact structures. Dr. Huntress briefed the members on the roles of NASA centers, plans for the creation of science institutes, and the downsizing of Office of Space Science staff at NASA Headquarters. Dr. Rahe (via teleconference) reported on the status of ongoing planetary programs and, in particular, on the recent selection of Lunar Prospector as the third Discovery mission. Dr. Alan Binder (Lockheed Martin), Lunar Prospector's principal investigator (PI), gave a detailed briefing on this mission, its background, and likely scientific achievements. Discussion of the Discovery program continued in a conversation with Dr. Richard Goody (Harvard University), PI for the proposed Venus Multiprobe Mission.

The committee next met in Washington, D.C., on September 6-8 to hold initial briefings relating to a number of potential studies. Its new chair, Dr. Ronald Greeley, gave a brief presentation on the committee's role and position within the Board and the NRC. Most of the meeting was used for planning for, and presentations on, possible future studies on the exploration of the trans-Neptune solar system, the role of mobility in planetary exploration, and the exploration of Earth-approaching asteroids. Dr. William Piotrowski of NASA's Solar System Exploration Division gave a briefing on current and future planetary science missions and on the status of NASA's space science budget.

Later in the day, Dr. Giulio Varsi of the Solar System Exploration Division briefed the committee on new planetary missions, such as the proposed Pluto Express, and the technologies needed to perform them. Other presenters on the first day of the meeting included Drs. James Cutts and Kerry Noch of JPL on the use of balloons for planetary robotic exploration, and Dr. John Darrah of the USAF Space Command on its asteroid detection program.

On the second day of the meeting, Dr. William McKinnon of Washington University detailed the characteristics of solid bodies in the trans-Neptune region. Dr. Harold Levison of Southwest Research Institute discussed current understanding of the content of the Kuiper belt. Dr. Ralph McNutt of the Applied Physics Laboratory talked about the particle and fields environment in the outer heliosphere. Dr. Jonathan Lunine of the University of Arizona updated the committee on the deliberations of the Pluto Express Science Working Group. One important issue raised in Dr. Lunine's presentation was the question of ultimate management authority in a mission using the "science-craft" approach (i.e., spacecraft systems designed around an integrated instrument package rather than the conventional approach of integrating individual instruments into a predesigned spacecraft bus) adopted by that working group. On the final day of the meeting, the committee heard from Dr. Richard Binzel of the Massachusetts Institute of Technology on current scientific understanding of near-Earth objects.

COMPLEX Membership

Ronald Greeley, Arizona State University (chair)
 Joseph A. Burns,* Cornell University (former chair)
 James R. Arnold, University of California at San Diego
 Frances Bagenal, University of Colorado at Boulder
 Jeffrey R. Barnes, Oregon State University
 Geoffrey A. Briggs,* NASA Ames Research Center
 Michael H. Carr,* U.S. Geological Survey
 Philip R. Christensen, Arizona State University
 Russell Doolittle, University of California at San Diego
 James L. Elliot,* Massachusetts Institute of Technology
 Heidi Hammel, Massachusetts Institute of Technology
 Barry H. Mauk,* Johns Hopkins University
 George McGill, University of Massachusetts
 William B. McKinnon,* Washington University
 Harry McSween, Jr., University of Tennessee
 Ted Roush, San Francisco State University
 John Rummel, Marine Biological Laboratory
 Gerald Schubert, University of California at Los Angeles
 Eugene Shoemaker, U.S. Geological Survey
 Darrell F. Strobel, Johns Hopkins University
 Alan T. Tokunaga, University of Hawaii
 George W. Wetherill,* Carnegie Institute of Washington
 Roger Yelle, University of Arizona
 Maria T. Zuber, Johns Hopkins University

David H. Smith, Study Director
 Altoria B. Ross, Administrative Assistant

*term ended during 1995

COMMITTEE ON SPACE BIOLOGY AND MEDICINE

The Committee on Space Biology and Medicine (CSBM) met on April 12-14 in Washington, D.C., to consider implications of the NASA Federal Laboratory Review (also known as the Foster Committee report), with emphasis on issues related to peer review in the life sciences. During the course of the meeting, CSBM was given an overview of the NASA life sciences program by Director Joan Vernikos, of NASA's Life Sciences and Applications Division (LSAD). Deputy Director Frank Sulzman briefed the committee on life sciences research programs, and Dr. Earl

Ferguson, director of the Aerospace Medicine Division, was present to answer questions. Dr. Ronald White, also of the LSAD, gave a presentation on peer review activities, and Associate Administrator Harry Holloway briefed the committee on NASA's restructuring actions. On the second day of the meeting the committee developed a draft of a letter report on peer review and the NASA Federal Laboratory Review; the letter was later approved by the Board at its June meeting (Section 4.4).

The committee met on July 27-28 at Woods Hole to develop a detailed work plan for a major review of the NASA life sciences program. This task is envisioned as a survey of all the life sciences areas appropriate for study in a space environment and will replace the committee's last major strategy report, published in 1987. The meeting began with opening remarks by Chair Mary Jane Osborn on the committee's recently completed letter report on peer review. The letter was formally presented to Dr. Vernikos later in the afternoon. Committee member Richard Setlow gave a presentation on space radiation issues, and there was a discussion of the committee's planned study on this topic. Dr. Vernikos and other LSAD representatives agreed that such a study would be useful to NASA. Dr. Vernikos then briefed the committee on research status, planning for new NASA science institutes, and international collaboration planning. Drs. Ronald White, Walter Schimmerling, and Frank Sulzman gave presentations on the range of life sciences research currently sponsored by NASA. On the morning of the second day of the meeting there was a general discussion of the topics presented by NASA and of various options for carrying out the strategy task. Dr. Vernikos offered the assistance of NASA in supporting future workshop activities. The committee met in executive session after lunch and developed a detailed plan for carrying out the study.

The committee met again on October 23-25 in Washington, D.C., to continue work on the major strategy review for NASA life sciences. After opening remarks by the chair, the committee heard overviews of life sciences discipline areas presented by individual committee members and outside experts to introduce committee members to the status of research in those fields of the life sciences relevant to space investigation. Several observers were present from NASA Headquarters to comment on the points raised in the presentations. Mr. Alphonso Diaz, NASA deputy associate administrator for space science, gave the committee an overview of NASA's current planning for the science institutes. The committee noted the rapid pace at which NASA was proceeding to set up the life sciences institutes and the limited planning detail available. The committee entered executive session on the second day to refine its three-year work plan for the research strategy project. These refinements included a decision on the structure and outline of the report. Much of the detailed planning focused on the committee's upcoming February 1996 workshop in cell biology, which will be chaired by member Elliot Meyerowitz.

CSBM Membership

Mary Jane Osborn, University of Connecticut Health Center (chair)
 Norma M. Allewell, University of Minnesota
 Charles J. Arntzen, Texas Medical Center
 Robert E. Cleland, University of Washington
 Mary F. Dallman, University of California at San Francisco
 Francis (Drew) Gaffney, Vanderbilt University
 Marc D. Grynbas,* Samuel Lunenfeld Research Institute
 James R. Lackner, Brandeis University
 Anthony P. Mahowald, University of Chicago
 Robert W. Mann,* Massachusetts Institute of Technology
 Elliot Meyerowitz, University of Chicago
 Kenna D. Peusner, George Washington University Medical Center
 Steven E. Pfeiffer, University of Connecticut Health Center
 Gideon A. Rodan, Merck, Sharp and Dohme Research Laboratory
 Fred D. Sack,* Ohio State University
 Richard Setlow, Brookhaven National Laboratories

Sandra J. Graham, Study Director
 Shobita Parthasarathy, Research Assistant
 Victoria Friedensen, Administrative Assistant

*term ended during 1995

COMMITTEE ON SOLAR AND SPACE PHYSICS

The Committee on Solar and Space Physics (CSSP) met jointly with the Committee on Solar-Terrestrial Research (CSTR) at the Beckman Center on February 14-17. Chairs Janet Luhmann and Marvin Geller briefed the committees on the status of report activities and the Board's request for contributions to the Future of Space Science (FOSS) study. The recent release of NASA's FY96 budget and the absence from it of any new starts for space physics were major topics of discussion. Briefings on agency activities were given by four individuals. Dr. George Withbroe, NASA's division director for space physics, gave a detailed briefing via videoconference on NASA's FY96 budget. Dr. Richard Behnke, NSF, and Col. Tom Tascione, USAF, gave updates, also via videoconference, on the latest developments in the national space weather initiative. In addition, Dr. Behnke gave a brief account of NSF's FY96 budget and expressed interest in the idea of a short workshop on solar variability. Agency status reports were completed by Dr. Ronald Zwickl in a roundup of NOAA activities. The final briefing at the meeting was a status report on the Thermosphere-Ionosphere-Mesosphere Energetics and Dynamics (TIMED) mission by Dr. Samuel Yee of the Applied Physics Laboratory. The idea of sponsoring a workshop on TIMED, and its connection to the ongoing Upper Atmosphere Research Satellite mission, was raised. The workshop was tentatively scheduled for the fall. The main goal of the meeting was writing for the Board on Atmospheric Sciences and Climate's "21st Century Report." The remainder of the meeting was taken up with drafting material for this and the FOSS study and writing a letter to the chair of the Board on concerns raised by NASA's FY96 budget.

The two committees met jointly at the Beckman Center on August 1-4 for their annual summer meeting. The primary goal of the meeting was to complete writing assignments for the 21st Century Report. Other projects to be worked on included an assessment of the space physics aspects of NASA's Office of Space Science (OSS) strategic plan and a research briefing report on space weather. Following the completion of these studies, the committees anticipated undertaking a scientific assessment of TRACE and any space physics projects selected for development in the next round of midsize Explorer (MIDEX) missions. Dr. Behnke, NSF's division director for atmospheric sciences and co-chair with Col. Tascione of the National Space Weather Program interagency initiative, described (via videoconference) ongoing activities and, in particular, a June 15-16 meeting in Arlington, Virginia, convened to draft an implementation plan for a National Space Weather Program. NASA Associate Administrator for Space Science Wesley Huntress described (via videoconference) the ongoing Zero Base Review and headquarters reorganization. Central to the reorganization of the Office of Space Science is the creation of a single science division and a new science "board of directors." Current visiting senior scientist positions will be phased out, but IPA positions are likely to remain. Dr. Withbroe briefed the committees (via videoconference) on the latest developments in NASA's space physics program. One topic addressed was changes likely in the availability of launch opportunities. In particular, a formal "secondary payloads" NASA Research Announcement or Announcement of Opportunity may be issued in order to take advantage of piggyback launches. Dr. Withbroe also described some new mission concepts, including NASA participation in Japan's Solar-B mission and in a Solar Polar Imager designed to detect Earth-bound mass ejections.

The federated committees met together in Washington, D.C., on November 13-15. Prior to the furlough of U.S. government employees, which began November 14, several representatives of government agencies briefed the committees. Dr. Sarah Horrigan, a program examiner for science and space programs with the Office of Management and Budget, and Dr. Richard Obermann, a congressional staff member with the U.S. House of Representatives' Subcommittee on Space and Aeronautics, provided perspectives on the budget and the image of space research on Capitol Hill. In order to keep the committees abreast of recent activities relevant to the space weather research briefing, CSSP member Spiro Antiochos and Study Director David Smith told the committees about the recent space weather workshops they had attended, respectively, at the Naval Research Laboratory in September and at the Sacramento Peak Observatory in October.

CSTR member Jack Gosling gave a brief account of a recent meeting at which opposing champions in the Flare versus Coronal Mass Ejection controversy had presented their views. The next presentation was from Dr. Michael Teague, who gave an overview of the U.S. Solar-Terrestrial Energy Program coordinating activities following the reduction of support for that office at the Goddard Space Flight Center. In particular, he described the World Wide Web (WWW) pages currently under development that tie together some of the ground-based contributions to the International Solar-Terrestrial Physics (ISTP) program and Interagency Consultative Group (IACG) for space science programs. NASA Space Physics Division Director Withbroe reviewed the new NASA Headquarters organizational chart. Dr. Withbroe said that the new organization, to be implemented soon, will place him in a new

council of science advisors to the associate administrator, where he will be responsible for the theme “Sun-Earth-Heliosphere Connection.” He will be one of four directors who will interface with the new, integrated Science Division under Mr. Henry Brinton. Dr. Withbroe said that space physicists should see several new activities in 1996, including the appearance of a NASA research announcement on “New Mission Concepts” (for support of mission concept development), and activities related to formulating a discipline mission strategy for NASA’s long-range planning exercise to be held in 1997. Next, Dr. Behnke (NSF) and Col. Tascione (USAF) described the status of the National Space Weather Program initiative, which was going forward with a multiagency announcement of opportunity expected in 1996. A “fast prototyping laboratory” where investigators can try out space weather tools is envisioned as part of this program. Col. Tascione stressed the need for good metrics of progress and solicited ideas for them. Dr. John Lynch, from the NSF Office of Polar Programs, described the new organization of space physics activities, with he himself responsible for Antarctic programs and Dr. Odile de la Beaujardiere assuming a new position of responsibility for Arctic space physics-related programs (among others). Polar programs of concern to the two committees include helioseismology, neutron monitors, wave transmission experiments, radar installations, and balloon programs. Dr. Chris Russell (UCLA) reviewed advantages of solar electric propulsion (SEP) for space physics missions. SEP is expected to become available as a mission design option following testing on the recently announced first New Millennium mission (launch in 1998).

During writing sessions, the committees completed the *Opportunities, Challenges and Imperatives in Upper Atmosphere Research Entering the 21st Century* for the Board on Atmospheric Sciences and Climate (the parent board of CSTR) and the final draft of a short report assessing space physics aspects of NASA’s 1995 Office of Space Science’s *Space Science Enterprise Strategic Plan—Space Science for the 21st Century*. This committee assessment was based on a comparison with the committees’ own recently completed *A Science Strategy for Space Physics* (Section 3.9).

CSSP Membership

Janet G. Luhmann, University of California at Berkeley (chair)
 Spiro K. Antiochos, Naval Research Laboratory
 Janet U. Kozyra, University of Michigan
 Robert P. Lin, University of California at Berkeley
 Donald G. Mitchell, Johns Hopkins University
 Arthur D. Richmond, National Center for Atmospheric Research High Altitude Observatory
 Harlan E. Spence, Boston University
 Michelle F. Thomsen, Los Alamos National Laboratory
 Roger K. Ulrich, University of California at Los Angeles

David H. Smith, Study Director
 Altoria B. Ross, Administrative Assistant

*term ended during 1995

PROJECT ON THE FUTURE OF SPACE SCIENCE

The Future of Space Science (FOSS) project is a cluster of related studies responding to FY94 Senate appropriations report language and a subsequent request by NASA Administrator Daniel Goldin. The Senate report language stated the following:

The future of space science—The Committee has included \$1,000,000 for the National Academy of Sciences to undertake a comprehensive and independent review of the role and position of space science within NASA. It will come as no surprise that the Committee did not support or recommend the dismantling of the Office of Space Science and Applications. The contributions made by that office in strategic planning, cross disciplinary priority setting, and management controls were among the best that the Federal Government has ever undertaken in any of its many scientific components. Given the administration’s desire to reinvent Government, the Committee believes the time has come to

seriously consider the creation of an institute for space science that would serve as an umbrella organization within NASA to coordinate and oversee all space science activities, not just those in physics, astronomy, and planetary exploration. Such an institute could function just as the National Institutes of Health now does within the Department of Health and Human Services. The Committee recognizes that there are certain tradeoffs in the creation of any new entity. The Academy should look at mechanisms for priority setting across disciplines on the basis of scientific merit, better means to include advanced technology in science missions, and ways to permit less developed scientific disciplines to have a means of proving their value, despite skepticism about them in the more established scientific fields.

Additional guidance was provided in the FY95 appropriations report:

The future of space science—The Committee is concerned that no new space science missions are now planned to be launched by NASA after 1997 at this time. In addition, it is deeply troubled by reports that a so-called wedge of funding in the 1996 budget for any new science flight projects may require one-half of the funds to come from existing science budgets. Neither condition is acceptable, and the Committee will expect whatever pool of funds to be used for future new starts to come outside of the existing base of space science funds. The Committee expects the National Academy of Sciences to factor this funding and mission vacuum into its assessment for the need for a national institute for space science.

FOSS Steering Group

The FOSS Steering Group met on January 4-5 in Washington, D.C. The meeting began with a general discussion of the FOSS study. It was noted that policy environment changes had broadened the scope of the study and might increase interest in and acceptance of its results. With a smaller NASA budget likely in the future, it was decided that the study should focus on the distinct role of science in the space program. This should include the issue of quality in a budget environment that has stopped expanding.

In remarks to the steering group, NASA Administrator Daniel Goldin described his own perspective on the status of science within the agency. He stated that “the true job of NASA is not to support the scientific community” and that success cannot be measured by the number of scientists or by the dollars allocated to them. He expressed satisfaction with the present science organization, as well as the thought that setting up a separate space science institute would not be in the best interest of the agency. He favored “Darwinism,” referring to survival of only the strongest programs. He suggested that major areas for the study to address were (1) the peer review process, (2) procurement with universities, and (3) prioritization, given the need to cancel some programs. According to Mr. Goldin, the most constructive thing the committee could do would be to “establish that it is socially acceptable to cancel an ongoing program to start something new.” Chief Scientist France Cordova remained to continue the discussion after Mr. Goldin was obliged to leave the meeting.

Status was reported for each of the FOSS task groups. FOSS Task Group on Technology (FOSS-T) Co-Chairs John Hedgepeth and Anthony England reported on the November meeting of that task group. Although technology utilization had formally become a figure of merit for projects, the agency’s integrated technology planning did not appear adequate. The task group perceived that the Office of Space Access and Technology was trying to follow an Advanced Research Projects Agency (ARPA) model, which, if coordinated as an approach to goals, would be an improvement. FOSS Task Group on Research Prioritization (FOSS-RP) Chair Roland Schmitt presented some preliminary thoughts of his task group that he expected to develop further during a teleconference planned for the following week. He pointed out that this task group would look to see which pieces of a candidate process being considered for endorsement already existed within NASA. The 1991 Office of Space Science and Applications “Woods Hole process,” as well as the second report of the Task Group on Priorities in Space Research (in press), would be examined for components that could be included. FOSS Task Group on Alternative Organizations (FOSS-AO) Chair Daniel Fink reported that his group planned to hear from numerous individuals and organizations on various organizational structures. Given the various forces acting on the present agency, he felt that the committee would probably need to broaden its study beyond the narrow issues in the Senate report language.

FOSS Chair John Armstrong led further discussion of the deliberations to date. The group should lay out an understanding of what NASA science should be; what a healthy and balanced program is, including the whole science life cycle; and what the proper relation should be between science and engineering throughout this life cycle. Reservations were expressed as to whether selection and priority setting were really “broken” *within*

disciplines, or only *between* them. Further discussion contrasted mission definition and PI funding at NASA with those processes at the National Institutes of Health (NIH). The meeting ended with assignment of tasks to members and staff.

The steering group reconvened on June 9-10, joining the Space Studies Board in joint session on June 9. During the joint session, steering group and task group chairs briefed the Board on early findings of the project and obtained the Board's inputs. The remainder of the meeting was used by the steering group to conduct a detailed review of the work of the three FOSS task groups; the results gave the task group chairs guidance for completing their reports. On the afternoon of June 9, Administrator Goldin again met with the steering group and gave his perspective on current events at the agency. He also requested that delivery of the final report of the project be advanced from January 1996, as originally planned, to September 1995. Acknowledging the challenge, Chair Armstrong agreed to expedite the work and to strive for a final briefing in late September.

The FOSS project thus greatly accelerated its schedule. To meet the new September completion date for the report, an integrated draft was assembled and sent to the steering group on July 14. Subsequently, an improved draft was circulated to the full Board for review. The steering group convened for the last time on August 7 and 8 at Woods Hole. The first day and a half were spent revising the draft report in response to suggestions obtained from the entire Board in late July. After making a number of changes and resolving a number of unsettled issues, the steering group submitted the amended report to the executive committee of the Board for approval on the afternoon of August 8. The executive committee approved this draft, which was submitted to NRC institutional review a few days later. Following revision and formal approval by the NRC, the final report, *Managing the Space Sciences*, was completed on September 22 (Section 3.6). FOSS Committee Steering Group Chair Armstrong, FOSS Task Group on Alternative Organizations Chair Fink, and Board Chair Claude Canizares briefed Administrator Goldin and senior NASA management on the findings of the report on September 26, and the Board staff began initial distribution of the document. In all, the 35 members of the four FOSS groups met 21 times over a 12-month period to complete the study.

FOSS-SG Membership

John A. Armstrong, IBM Corporation (retired) (chair)
 Anthony W. England, University of Michigan
 Daniel J. Fink, D.J. Fink Associates, Inc.
 Ursula W. Goodenough, Washington University
 John M. Hedgepeth, Digisim, Inc.
 Jeanne E. Pemberton, University of Arizona
 William Press, Harvard-Smithsonian Center for Astrophysics
 P. Buford Price, University of California at Berkeley
 Roland W. Schmitt, Rensselaer Polytechnic Institute (retired)
 Guyford Stever (retired)
 James Wyngaarden, National Institutes of Health (retired)

Marc S. Allen, Study Director
 Betty C. Guyot, Administrative Officer
 Carmela J. Chamberlain, Administrative Assistant
 Nathaniel B. Cohen, Consultant

FOSS Task Group on Alternative Organizations

The FOSS Task Group on Alternative Organizations (FOSS-AO) met on February 2-3 in Washington, D.C., where the group heard briefings from officials of many government and government-related agencies on organizational structures. Dr. Robert Cooper, a member of the task group and former director of ARPA, described ARPA's history and operating principles. Dr. Anita Jones, director of Defense Research and Engineering, described the structure and decision-making hierarchy within the Department of Defense (DoD). Mr. Robert Winokur, NOAA/NESDIS, described the NOAA structure and the future distribution of responsibilities between NOAA, NASA, and DoD on NPOESS. Dr. Ruth Kirschstein, deputy director of the NIH, described the 24-institutes system of the NIH.

Dr. Stamatios (Tom) Krimigis, space science director for the Applied Physics Laboratory, presented an overview of the laboratory. Dr. Robert LaFande, Naval Research Laboratory (NRL), described the NRL as a very hands-on operation, with an almost medieval guild character among the staff, ranging from apprentice to master craftsman. Mr. Ian Pryke, head of the European Space Agency's (ESA) Washington office, briefed the task group on the structure of ESA. Dr. Harry Holloway, NASA associate administrator for life and microgravity sciences and applications, talked about moving the life and microgravity sciences into the Human Exploration and Development of Space Enterprise in NASA's strategic plan, and out of the Scientific Research Enterprise, which has been renamed the Space Science Enterprise. Dr. John Klineberg, director of Goddard Space Flight Center (GSFC), spoke concerning space science activities at GSFC. Mr. Alan Ladwig, NASA associate administrator for the Office of Policy and Plans, spoke about the purpose and function of this new office.

The task group's second meeting was on March 10-11 at the Beckman Center, where the group continued to hear briefings on agencies' structures. Mr. Ray Colladay of Lockheed Martin described ideas for revitalizing NASA's technology program. Dr. Martin Blume, a member of the task group and deputy director of Brookhaven National Laboratory, described the science management system in the Department of Energy. During executive session, the group discussed problems in NASA's science organization and management. Recommendations that would use NASA's current downsizing to the agency's advantage were identified. The meeting was concluded by making writing assignments and deciding whom to invite to the next meeting.

The third meeting was held on March 27-28 in Washington, D.C. Dr. Edward Stone, director of the Jet Propulsion Laboratory (JPL), briefed the group on the JPL infrastructure and its relationship with NASA. Dr. John Mansfield, NASA associate administrator for space access and technology, briefed the committee on the Space Technology Enterprise Integrated Technology Plan. Dr. Walter Morrow, director of the Massachusetts Institute of Technology's Lincoln Laboratory, described the laboratory, whose mission is to develop new technology for DoD. Dr. William Miller, currently at West Virginia University and previously director of the Office of Naval Research and commander of Naval Research, described the relationship between the Navy scientific headquarters organization and its field centers. Dr. Lew Allen, past director of JPL and currently a director of the Draper Laboratory, addressed the committee by videoconference regarding the JPL model for a federally funded research and development center (FFRDC) laboratory. Dr. Lennard Fisk, previously NASA associate administrator for space science and applications, gave his perspective on past and possible future NASA science organizations. NASA Deputy Administrator John Dailey discussed the reengineering efforts at NASA. During executive session, the group continued listing perceived problems, the pros and cons of the present science organization, and the respective responsibilities of NASA Headquarters and its field centers. There was also some discussion regarding the NASA internal "red team white paper." The group discussed the pros and cons of the JPL model and an institute model. Principles important to science organization and management were identified, and the meeting concluded with the delegation of additional writing assignments to members and staff.

The task group met twice during the second quarter. At an April 18-19 meeting in Washington, D.C., the task group discussed several issues of importance to establishing a workable organizational structure. Among these issues were the roles of the chief scientist, Science Council, institutes, and technology. Dr. John Naugle joined the task group by teleconference to report on his experiences as former chief scientist at NASA. He presented a brief overview of the position and left the committee with several questions to consider. Dr. Timothy Coffey from the Naval Research Laboratory was also in attendance and presented his view of how privatization of NASA science could be undertaken. His thoughts were based on a past analysis of potentially privatizing NRL. More writing assignments were made at the end of the meeting.

The task group met again on May 24-25 in Washington, D.C. NASA Chief Scientist France Cordova, Dr. Harry Holloway, and Associate Administrator for Mission to Planet Earth Charles Kennel joined the committee to review the results of NASA's Zero Base Review team. In addition, the task group discussed details of the report and began to assemble the full text of recommendations and observations. Completion of its contribution to the final report was accomplished via teleconferences and electronic communication.

FOSS-AO Membership

Daniel J. Fink, D.J. Fink Associates, Inc. (chair)
Martin Blume, Brookhaven National Laboratory
John F. Cassidy, United Technologies Research Center

Robert S. Cooper, Atlantic Aerospace Electronics Corporation
 Gerald P. Dinneen, National Academy of Engineering
 Harold B. Finger, General Electric Company (retired)
 Noel W. Hinnners, Lockheed Martin Astronautics
 Thomas E. Malone, Association of American Medical Colleges (retired)
 Norine E. Noonan, Florida Institute of Technology
 James R. Thompson, Jr., Orbital Sciences Corporation
 Arthur B.C. Walker, Jr., Stanford University
 Sidney C. Wolff, National Optical Astronomy Observatories

Marc S. Allen, Study Director
 Richard C. Hart, former Study Director
 Erin C. Hatch, Research Assistant
 Carmela J. Chamberlain, Administrative Assistant
 Nathaniel B. Cohen, Consultant

FOSS Task Group on Research Prioritization

The FOSS Task Group on Research Prioritization (FOSS-RP) held a teleconference on January 9 to discuss the status of the conclusions reached to date. Notes on the hierarchy of priority and goal setting, as well as on the processes at each step, were prepared by Chair Roland Schmitt and circulated to the group a few days in advance. The group used these notes and expanded on them by discussing the ordering of priorities and criteria that should be used at each step. The teleconference ended with tasks assigned to members and staff.

On February 21-22 the task group convened in Washington, D.C. Mr. Alan Ladwig briefed the group on objectives of the Office of Policy and Plans in NASA's reinvention process. During executive session, there was much discussion contrasting priority setting within disciplines with priority setting on an interdisciplinary and agency-wide basis. The role of the Board in setting priorities was discussed at length, including what importance should be given at this level to mission costs. The group developed lists of possible criteria for the use of disciplinary committees and factors related to the composition of committees that establish goals. Previous NASA priority-setting methods, including the 1991 "Woods Hole Process," were examined and contrasted with the present situation, and the group developed criteria that would apply to a NASA-level prioritization. The meeting concluded with a discussion on differences among the six NASA scientific disciplines. It was decided that different disciplines might necessitate different methods for prioritization. Writing assignments for summary papers on how past disciplinary priority efforts worked were given to members and staff.

The task group gathered for a final meeting on June 5-6 in Washington, D.C. The goal of the meeting was to develop an organizational system that would prioritize science projects and goals while also taking into account other political and social factors. At the beginning of the meeting Chair Schmitt presented a general outline for a four-tier organization suitable to such a process. Modification of the proposal continued as the task group evaluated the full practicality of such a general system and put it through several tests. The task group used teleconferences and electronic mail to finalize its proposals and recommendations.

FOSS-RP Membership

Roland W. Schmitt, Rensselaer Polytechnic Institute (retired) (chair)
 William F. Brinkman, AT&T Bell Laboratories
 Larry W. Esposito, University of Colorado at Boulder
 Robert A. Frosch, Harvard University
 David J. McComas, Los Alamos National Laboratory
 Christopher F. McKee, University of California at Berkeley
 Morton B. Panish, AT&T Bell Laboratories (retired)
 Carlé M. Pieters, Brown University
 Rudi Schmid, University of California at San Francisco
 Eugene B. Skolnikoff, Massachusetts Institute of Technology

Marc S. Allen, Study Director
Richard C. Hart, former Study Director
Erin C. Hatch, Research Assistant
Carmela J. Chamberlain, Administrative Assistant
Nathaniel B. Cohen, Consultant

FOSS Task Group on Technology

The FOSS Task Group on Technology (FOSS-T) met at JPL on February 23-24, the first of three meetings at NASA centers intended to educate the group about NASA's processes for developing new technology for space science and for inserting new technologies into its missions. JPL's senior management participated in the meeting and responded to questions from the task group. JPL briefed the committee on technology requirements, technology development, technology infusion, technology transfer, and lowering the cost of flight projects, and provided tours of some of its special facilities. The JPL meeting provided an opportunity to learn about some new NASA programs that use new technology, such as the New Millennium and the Mars Pathfinder programs, and their relationship to the agency's increased emphasis on using new technology in its missions. Questions were raised about the sources of funding and expertise to develop the new technology for new-technology-based missions. A major issue discussed by JPL representatives and the task group was the disconnect between technology development projects started at the Office of Space Access and Technology and the science offices, especially the Office of Space Science. The meeting was concluded by JPL's mentioning that since the release of the NRC's report *Improving NASA's Technology for Space Science*, JPL has added a project technologist to its projects, owing partly to the recommendations made in the report.

The task group met at NASA's Lewis Research Center on March 30-31 for the second of its three planned visits to NASA centers. The purpose of the visit was to learn about the technology work sponsored there by the Office of Life and Microgravity Sciences and Applications and the Office of Space Access and Technology. The task group was introduced to the capabilities of the center by Director Donald Campbell and received a day and a half of briefings and tours that concentrated on Lewis' traditional strengths in space technology (power, communications, and propulsion) and microgravity sciences (especially transport phenomena, fluid mechanics, and combustion).

During the second quarter, the task group met in Washington, D.C., and at the Goddard Space Flight Center on April 24-26, and again in Washington, D.C., on June 5-6. At the April meeting in Washington, D.C., the task group met with the associate administrators of the Offices of Space Science (Dr. Huntress), Life and Microgravity Sciences and Applications (Dr. Holloway), Mission to Planet Earth (Dr. Kennel), and Space Access and Technology (Dr. John Mansfield), along with other key officials. This meeting was held to discuss the planning and programs under way at NASA for developing and infusing new technology into NASA's space science missions and programs. The meeting at GSFC was among the task group members and the departing center director (Dr. John Klineberg) and his staff. This meeting was held in order to learn about the GSFC projects to develop technology for space science and Earth science missions. The June 5-6 meeting was an executive session for reaching a consensus on the report's main findings and recommendations. The task group completed its input for submission to the FOSS Steering Group on June 30.

FOSS-T Membership

Anthony W. England, University of Michigan (co-chair)
John M. Hedgepeth, Digisim, Inc. (co-chair)
Joseph P. Allen, Space Industries International, Inc.
Robert P. Caren, Lockheed Corporate Headquarters
John J. Donegan, John Donegan Associates, Inc.
James W. Head III, Brown University
John M. Logsdon, George Washington University
Simon Ostrach, Case Western Reserve University
Judith Pipher, University of Rochester
Alfred Schock, Orbital Sciences Corporation

Henry Plotkin, Technical Advisor

Noel E. Eldridge, Study Director
Carmela J. Chamberlain, Administrative Assistant

TASK GROUP ON BIOLOGICAL EFFECTS OF SPACE RADIATION

The Task Group on Biological Effects of Space Radiation, a subpanel of the Committee on Space Biology and Medicine, held its first meeting on November 13-15, in Washington, D.C., to begin work on its task to identify the research needed to reduce the risk and uncertainty of radiation hazards to humans on extended, deep-space missions. After opening remarks by task group Chair Richard Setlow, the general session began with a short talk by Dr. Harry Holloway, associate administrator for life and microgravity sciences and applications, regarding the type of information NASA hoped to obtain from the study. Dr. Holloway stressed the need for a concrete research plan, with timetables, that would allow NASA to be ready for a piloted mission to Mars in the year 2018. Both this presentation, and one given by NASA's Deputy Director of Life Sciences Frank Sulzman, helped the task group sharpen the focus of its task. Dr. Walter Schimmerling, of NASA's Life Sciences and Applications Division, gave a presentation on the design of NASA's radiation research program. The task group also heard several presentations on the status of NASA radiation research and the agency's current understanding of radiation hazards.

The task group went into executive session at midmorning of the second day to discuss various technical issues relating to research in this field, including an apparent controversy among radiation investigators about the validity and applicability of various published data sets. The task group agreed on a detailed outline of the report, as well as a preliminary list of areas of recommended research. Writing assignments were made, with provisions for sections of the report to be drafted in the coming weeks and integrated prior to the next meeting. Task group members would confer with each other and members of the research community regarding the available data during this drafting phase. A tentative schedule of meetings was agreed on.

TGBESR Membership

Richard Setlow, Brookhaven National Laboratory (chair)
John F. Dicello, Jr., Johns Hopkins University School of Medicine
R.J. Michael Fry, Oak Ridge National Laboratory
John B. Little, Harvard School of Public Health
R. Julian Preston, Chemical Industry Institute of Toxicology
James B. Smathers, University of California at Los Angeles
Robert L. Ulrich, University of Texas Medical Branch at Galveston

Sandra J. Graham, Study Director
Shobita Parthasarathy, Research Assistant
Victoria Friedensen, Administrative Assistant

TASK GROUP ON PRIORITIES IN SPACE RESEARCH

Revision of the final report of the Task Group on Priorities in Space Research (TGPSR), *Setting Priorities for Space Research: An Experiment in Methodology*, was completed, and the report was published and disseminated (Section 3.8).

TGPSR Membership*

John A. Dutton, Pennsylvania State University (chair)
Philip H. Abelson, American Association for the Advancement of Science
William P. Bishop, Desert Research Institute
Lawson Crowe, University of Colorado at Boulder
Peter Dews, Harvard Medical School (retired)
Angelo Guastaferrro, Lockheed Missiles and Space Company, Inc.
Molly K. Macauley, Resources for the Future

Thomas A. Potemra, Johns Hopkins University
Arthur B.C. Walker, Jr., Stanford University

Marc S. Allen, Study Director
Joyce M. Purcell, former Study Director
Carmela J. Chamberlain, Administrative Assistant

*task group disbanded in 1993

TASK GROUP ON THE BMDO NEW TECHNOLOGY ORBITAL OBSERVATORY

The Task Group on BMDO's New Technology Orbital Observatory (TGBNTOO) completed its report, *Scientific Assessment of a New Technology Orbital Telescope*, which was published in mid-September and distributed to interested parties in the DoD, NASA, and the scientific community (Section 3.5).

TGBNTOO Membership*

Michael F. A'Hearn, University of Maryland (chair)
Roger Angel, University of Arizona
Anita Cochran, University of Texas at Austin
James L. Elliot, Massachusetts Institute of Technology
Christ Ftaclas, Hughes Danbury Optical Systems
Garth D. Illingworth, University of California at Santa Cruz

Holland C. Ford, Johns Hopkins University, liaison from Space Telescope Science Institute

David H. Smith, Study Director
Altoria B. Ross, Administrative Assistant

*task group disbanded in 1995

TASK GROUP ON GRAVITY PROBE B

Formed with close cooperation by the NRC Board on Physics and Astronomy, the Task Group on Gravity Probe B (TGGPB) met three times on a fast-track schedule. The task group first met at Stanford University on January 10-12 to begin its review of the NASA general relativity mission Gravity Probe B (GP-B). The initial meeting included extensive briefings by Drs. Francis Everitt, Bradford Parkinson, and other members of the Stanford-Lockheed team on scientific and technical aspects of the proposed GP-B mission. Guest speakers Drs. Kip Thorne and Irwin Shapiro provided additional comment via videoconference on the scientific significance of the mission. In addition to receiving briefings, the task group toured facilities at both Stanford and Lockheed.

The second meeting of the task group was held in Washington, D.C., on February 10-11. At this meeting, NASA Administrator Goldin described his expectations for the study and how he intended to use the advice provided by the task group. Drs. Charles Townes, Roger Blandford, David Wilkinson, and Doyal Harper spoke to the task group about other NASA missions, such as SIRTf, SOFIA, and COBE. Drs. Everitt and Parkinson responded to new technical questions that had arisen following the January review.

On March 3-4 the task group met in Washington, D.C., to prepare its final report. Following a presentation by Dr. B. Lange on an experiment concept proposed as an alternative to GP-B, the task group discussed the issues in its charge and drafted a preliminary document describing its response.

The final report, *Review of Gravity Probe B*, was delivered to NASA Administrator Daniel Goldin on May 15, and task group Co-Chairs Val Fitch and Joseph Taylor and Space Studies Board Chair Claude Canizares briefed Mr. Goldin and key administrators (Section 3.3).

TGGPB Membership*

Val L. Fitch, Princeton University (co-chair)

Joseph H. Taylor, Jr., Princeton University (co-chair)
Eric B. Adelberger, University of Washington
Gerard W. Elverum, Jr., TRW Space and Technology Group (retired)
David G. Hoag, C.S. Draper Laboratory (retired)
Francis E. Low, Massachusetts Institute of Technology
John C. Mather, NASA Goddard Space Flight Center
Richard E. Packard, University of California at Berkeley
Robert C. Richardson, Cornell University
Stuart L. Shapiro, Cornell University
Mark W. Strovink, University of California at Berkeley
Clifford M. Will, Washington University

Ronald D. Taylor, Study Director
Susan G. Campbell, Administrative Assistant
Angela C. Logan, Project Assistant

*task group disbanded in 1995

3

Summaries of Major Reports

3.1 A Strategy for Ground-Based Optical and Infrared Astronomy

A Report of the Committee on Astronomy and Astrophysics,
under the aegis of the Board on Physics and Astronomy¹

EXECUTIVE SUMMARY

Astronomy occupies a special place in the research portfolio of this country. Understanding the cosmos is one of the oldest intellectual goals of humanity, and the discoveries of astronomers clearly excite the imagination of the public at large. From primary schools to universities, from planetaria to features in the media, astronomy offers numerous opportunities to improve the scientific literacy of this nation, and astronomers are increasingly engaged in these educational activities.

Although for many people astronomy is a clear example of one of the noblest of basic research activities, it is often less recognized that it can and does contribute to other national goals. In particular, its research activities depend on and contribute to the applied development of sophisticated sensors, an essential enabling technology for many scientific fields and for the defense, medical, and commercial sectors.

Modern astronomical facilities, and their sophisticated instrumentation, utilizing state-of-the-art detectors, computing resources, and optical design, are expensive. Astronomers are fortunate that the Congress has authorized the construction of numerous major national facilities. National ground-based astronomical facilities are supported primarily by the National Science Foundation (NSF), both in the construction and operations phases. The two 8-meter telescopes of the international Gemini 8-M Telescopes Project (IGP), in which the United States is a 50% partner, are currently under construction and will be completed by the end of the decade. Considerable investment (more than \$250 M in the past decade) in large telescopes has also been made with nonfederal support, such that private observatories now provide 81% of the total telescope area (and 76% of the net diameter) available to U.S. astronomers. Still, roughly half of U.S. astronomers must rely entirely on the National Optical Astronomy Observatories (NOAO) for access to telescopes, and nearly all rely on NOAO facilities for some aspects of their work.

The Panel on Ground-Based Optical and Infrared Astronomy was convened to determine whether the strategic balance of support by the NSF for all of optical and infrared (OIR) astronomy should be adjusted as these giant new telescopes come on line. In particular, the panel was asked to articulate a new mission for NOAO. In doing so, the

¹"Executive Summary" reprinted from *A Strategy for Ground-Based Optical and Infrared Astronomy*, National Academy Press, Washington, D.C., 1995, pp. 1-2.

panel had to address several complex questions. What is the best role for NOAO in U.S. participation in the IGP? How can the unique resources of both private and NOAO facilities best be deployed? What priorities and strategies should be pursued, recognizing that NSF resources for OIR astronomy will probably be severely constrained?

The panel believes that first priority must be given to the development of unique telescopes and instrumentation that advance technology and provide resources of national scope. The Gemini telescopes, the large telescopes at the Cerro Tololo Inter-American Observatory (CTIO), and the Advanced Technologies and Instrumentation (ATI) program of the NSF's Division of Astronomical Sciences are clearly in this category.

The panel finds that the case for increased OIR funding is strong within NSF for operating the Gemini telescopes. However, it is necessary to face the possibility that NSF funding of OIR astronomy will remain level in real dollars for some time. In this eventuality, the panel recommends that the proper instrumentation and operation of the Gemini telescopes should have first priority. The panel also affirms the high priority for the ATI program, which was recommended by the Astronomy and Astrophysics Survey Committee (AASC) report (*The Decade of Discovery in Astronomy and Astrophysics*, National Academy Press, Washington, D.C., 1991).

The panel concludes that, with level funding, major reductions in NOAO operations would be required to meet the priorities stated above. In this constrained situation the Tucson scientific, administrative, and technical services support would have to be scaled back very substantially. The level of support and convenience offered to observers would have to be reduced, and it is very likely that the smaller telescopes at the Kitt Peak National Observatory (KPNO) would need to be closed or privatized. Moreover, to reduce operations costs, the 4-meter Kitt Peak telescope would have to be operated with fewer instruments and used primarily for wide-field or near-infrared applications. In this case, a large number of astronomers whose only access to front-line research tools is through NOAO telescopes would be unable to carry out their research and U.S. science would suffer.

The panel has identified a strategy that might alleviate such problems and, at the same time, better utilize the very large recent expenditure by the private sector in the construction of new telescopes. Specifically, the panel recommends the initiation of a new program at a modest level within the NSF for instrumentation of the privately operated telescopes in exchange for national access. In a constrained budgetary scenario, such funds would, of necessity, come from existing NSF OIR astronomy activities, including the existing ATI program. Even with this new plan, some 1200 observer nights would be lost, approximately 40% of the present use by the U.S. astronomy community at NOAO nighttime facilities.

The above plan is the best that the panel can envision under a flat-budget scenario. But the panel finds the costs in human, educational, and scientific terms to be unacceptably high. In view of the major capital investments in the Gemini telescopes and other major new telescopes, the panel recommends a second strategy, contingent on the availability of additional funds. Specifically, the panel recommends that \$5.5 M/year be added to the NSF astronomy budget for international Gemini project operations. If this recommendation is implemented along with the proposed new instrumentation plan, it would allow for far more efficient utilization of existing telescopes. It would still be necessary to slim down the Kitt Peak/Tucson operations, but the consequences for the U.S. astronomy community would not be as draconian as they would be under the first strategy alone.

The panel recommends that a third strategy be pursued, if further funds are available. In this strategy, the NSF astronomy budget would be supplemented by \$10 M/year. The first \$5.5 M would be used as above for Gemini operations, and the balance would be used to support an augmented program for facility instrumentation grants. Independent observatories would be able to compete for these grants, which would be awarded strictly on the basis of scientific merit, but for which cost sharing, in the form of open access to the astronomical community at large, would be a requirement. Such a program would enable full utilization of the enormous investment in both federal and nonfederal capital in OIR telescopes.

The panel recognizes that when new, state-of-the-art facilities are brought on line, older facilities must be retired. All of the options outlined above include such painful downsizing. In the draconian, flat-budget scenario, the community would lose truly first-rate instruments, but even in the optimal plan, major economies in operations would still be required.

3.2 Microgravity Research Opportunities for the 1990s

A Report of the Committee on Microgravity Research¹

EXECUTIVE SUMMARY

INTRODUCTION

Microgravity research is concerned with the effects of reduced gravitational forces on physical, chemical, and biological phenomena. The scientific disciplines affected by gravity include fundamental physics, fluid mechanics and transport phenomena, materials science, biological sciences and biotechnology, and combustion. It is especially noteworthy that these disciplines are laboratory sciences that inherently use controlled, model experiments. Many experiments require constant attention and frequent intervention by the experimenter, which distinguishes microgravity research from the observational space sciences. Microgravity research also spans both fundamental and applied sciences.

Reduced-gravity experimental environments are provided by NASA through drop towers, aircraft in parabolic trajectories, sounding rockets, and Earth-orbiting laboratories. Some of these environments have crews and allow extended periods of time for experimentation and for demonstrating the reproducibility of results. Some of the experimental platforms allow a reduction of the gravity level to 10^{-6} times that of Earth gravity.

In a reduced-gravity environment, the decreases in rates of sedimentation, hydrostatic pressure, and buoyancy-driven flows cause other physical effects to become more important and more readily observable and measurable. The acceleration due to gravity can then be treated as an important and interesting experimental parameter. The exploration of this parameter, through experiments at normal Earth gravity and at reduced gravity, may provide a better understanding of certain physical processes, as well as lead to the identification of new phenomena.

The NASA microgravity research program has been widely misperceived as simply a materials processing program with the goal of providing better products for use on Earth. However, the prospects of commercial manufacture in space are limited in the foreseeable future. The justification for the microgravity research program must continue to be the promise of advances in areas of fundamental and applied science. It should be recognized, on the other hand, that manned exploration of space will require increased understanding of transport phenomena, materials processing, and performance in microgravity.

There have been a number of previous National Research Council (NRC) reports in this field, of which the following are particularly relevant:

- *Materials Processing in Space*² (the "STAMPS" report, 1978) attempted to provide guidance for the future course of NASA's program. It assessed the scientific and technological underpinnings of the materials processing in space program and provided a clear understanding of the potential for exploitation of the space environment for processing materials.
- *Space Science in the Twenty-First Century: Imperatives for the Decades 1995 to 2015. Fundamental Physics and Chemistry*³ (1988) attempted to identify opportunities for future research efforts in relativistic gravitation and microgravity science (but not including applied research or industrial/manufacturing processes).
- More recently, the report *Toward a Microgravity Research Strategy*⁴ (1992) began to lay a foundation for a more mature research program, and the current report is a continuation of that effort.

The research areas discussed in this report are subsets of much broader research disciplines. Only a small fraction of the total activities in each discipline occurs within the microgravity program, and no attempt has been made here to evaluate fully or to prioritize all of the research in a discipline. Only the microgravity component of

¹"Executive Summary" reprinted from *Microgravity Research Opportunities for the 1990s*, National Academy Press, Washington, D.C., 1995, pp. 1-19.

²Space Science Board, National Research Council, *Materials Processing in Space*, National Academy Press, Washington, D.C., 1978.

³Space Science Board, National Research Council, *Space Science in the Twenty-First Century: Imperatives for the Decades 1995 to 2015. Fundamental Physics and Chemistry*, National Academy Press, Washington, D.C., 1988.

⁴Space Studies Board, National Research Council, *Toward a Microgravity Research Strategy*, National Academy Press, Washington, D.C., 1992.

each discipline is addressed in detail. Furthermore, the cost-benefit of a microgravity program has not been compared to the cost-benefit of experiments on different subjects in the terrestrial environment. Experiments that can be performed adequately under terrestrial conditions, however, are not given a priority for spaceflight.

It should be understood at the outset that in evaluating the costs of doing research on such expensive vehicles as a space station, vehicle expenses are specifically not included. Microgravity research has never been the sole motivation for the space shuttle or other major space missions. These research opportunities have usually been secondary to exploratory, technological, engineering, political, educational, inspirational, and other motives for spaceflight, and until the advent of spacelab, microgravity research has been added on spaceflights launched for other purposes. Thus, in evaluating priorities, the research opportunities are taken into account but not the costs.

To date, only a limited number of microgravity experiments have been conducted in space with completed analyses and reports of results. The total experience of U.S., Canadian, Japanese, and Western European scientists is less than 1000 hours for experiments in orbit. Because of the limited results available, the strategic recommendations in this report cannot be highly detailed or exclusive. A number of subjects require further exploratory investigation before detailed objectives can be defined. For the same reasons, it is currently not generally possible to set specific priorities *across* discipline lines. Assuming experiments are scientifically sound, prioritization across disciplines is largely unnecessary because a wide range of microgravity experiments can be accommodated on shuttle flights without retarding progress in any one discipline. It may even be a mistake to attempt such prioritization since, in many cases, fundamental flight experiments are needed before prioritization can be attempted. Some areas, however, can be identified as more promising than others.

This report does not address the NASA commercial program or international programs in microgravity research. These topics will be addressed in future studies of this committee.

Finally, although the value and need for manned intervention capabilities and long-duration flights are noted repeatedly in this report, nothing herein should necessarily be interpreted as advocating or opposing any specific NASA spacecraft or space station design initiative.

SCOPE OF THE RECOMMENDATIONS

The conclusions and recommendations presented in this report fall into five categories: (1) overall goals for the microgravity research program; (2) general priorities among the major scientific disciplines affected by gravity; (3) identification of the more promising experimental challenges and opportunities within each discipline; (4) general scientific recommendations that apply to all microgravity-related disciplines; and (5) recommendations concerning administrative policies and procedures that are essential to the conduct of excellent laboratory science.

The overall goal of the research program should be to advance science and technology in each of the component disciplines. Microgravity research should be aimed at making significant impacts in each discipline emphasized. The purpose of this report is to recommend means to accomplish that goal.

The essential features of the recommendations are to emphasize microgravity research for its general scientific and technological value, as well as its role in advancing technology for the exploration of space; to deemphasize the research value of manufacturing in space with the intent of returning products to Earth; to modify NASA's infrastructure, policy, and procedures so as to facilitate laboratory science in space; to establish priorities for microgravity experimentation in scientific disciplines and subdisciplines in accordance with the relative opportunities for scientific and technological impact; and to recognize fluid mechanics and transport phenomena as a central theme throughout microgravity research. The recommendations detailed in this report are based on certain findings that resulted from this study. The findings are as follows:

- Science can be advanced by the study of certain mechanisms that are masked or dominated by gravitational effects at Earth gravity conditions. Examples include surface tension gradient-driven flows, capillary effects, multiphase flows, diffusive transport processes, and colloidal phenomena. Under terrestrial gravity conditions, these phenomena can often be dominated by effects such as buoyancy and sedimentation.
- There is a scientific need to understand better the role of gravity in many physical, chemical, and biological systems. To understand the relative importance of certain gravitational effects compared to nongravitational effects under terrestrial conditions, it is helpful to study a phenomenon at more than one gravitational level. Thus, gravitational force can become a controlled experimental parameter. For example, the role of buoyancy versus the role of surface tension gradients in driving a fluid flow could be examined at different gravitational levels.

- A significant portion of microgravity research programs should be driven by the technological needs of the overall space program. Examples of important engineering issues include the ignition, propagation, and extinction of spacecraft fires; the fluid dynamics and transport associated with the handling, storage, and use in space of water, waste, foods, fuels, air environments, and contaminants; the handling, joining, and reshaping of materials in space; and the dynamics and chemistry of mining or refining resources in extraterrestrial environments.

- Microgravity research primarily involves laboratory science with controlled model experiments that inherently require attention and intervention by the experimenter. Such experiments also require the opportunity to demonstrate reproducibility.

- The potential for manufacturing in space in order to return economically competitive products to Earth is very small. The techniques of space manufacturing, however, could prove to be important for materials science and materials processing on Earth.

- Fluid mechanics and transport phenomena represent both a distinct discipline and a scientific theme that impacts nearly all microgravity research experiments.

- The ground-based research program is critically important for the preparation and definition of the flight program.

The need for an extended-duration orbiting platform has been identified as critical in many microgravity research experiments because of the time required for experimentation, the wide parametric ranges, and the need to demonstrate the reproducibility of results. In developing this report, it has been assumed that a space station will be available within a decade. The recommendations have value and should be implemented, however, even if a space station does not materialize and the microgravity research program continues only with the current facilities.

Although this report does not set out a complete strategy for microgravity research, it presents some of the important elements of such a strategy, including:

- A summary of the current state of knowledge of microgravity science;
- A discussion of some of the fundamental questions to be answered;
- A presentation of the goals in this field;
- The science objectives within each discipline;
- An evaluation of the potential for microgravity research to provide advances within each discipline;
- The experimental requirements for achieving the science objectives of each discipline;
- A description of the other resources required for a successful microgravity science program; and
- A limited prioritization of research topics within each discipline.

The two aspects of a strategy for microgravity research that are not presented in this report are (1) a prioritization of microgravity research objectives across disciplines and (2) a cost-benefit analysis of anticipated microgravity results. Experience has shown how difficult it is to set research priorities, even within a single homogeneous science discipline. Reaching agreement on priorities for microgravity research relative to all other science research was judged so unlikely that it has not been attempted in this report. The other practical reason for not setting stricter priorities stems from the nature of shuttle flights. Frequently, payloads are assigned to a flight because their requirements for space, power, and so on, fit what is available, and scientific priority is less important. No cost-benefit analysis was attempted because the assumption is that orbiting platforms will be available for microgravity research. Decisions on the availability of platforms such as a shuttle or space station are essentially programmatic issues in which microgravity research is only one of many considerations.

RECOMMENDATIONS FOR THE SCIENCE PROGRAM

The disciplines of microgravity research discussed in this report include fluids and transport, combustion, biological sciences and biotechnology, materials science, and microgravity physics. Materials science is further categorized by constituent subfields: metals and alloys, organic materials and polymers, inorganic crystals, epitaxial layer growth, and ceramics and glasses. The expected degree of scientific success in each of these areas naturally varies.

General Priorities

- Fluids and transport, combustion, metals and alloys, microgravity physics, and certain areas of biotechnology offer the greatest likelihood for substantial advances. In these areas, focused experiments can be performed that will expand our knowledge of fundamental phenomena and processes.
- The areas of polymers, ceramics or glasses, inorganic crystals, and epitaxial layer growth, on the other hand, do not at present offer great promise of new advances. Limited support is still warranted, however, for certain fundamental studies.
- Certain areas of the biological sciences and biotechnology should be viewed from a different perspective. Much of the work in these areas is phenomenological. Such research should be pursued to discover the scope and potential for utilizing the microgravity environment.

Areas Recommended for Emphasis

Major prospects and opportunities in each of five disciplines are summarized in the following sections.

Fluid Mechanics and Transport Phenomena

Fluid mechanics and transport phenomena play a dual role in the microgravity research program. They stand as distinct disciplinary areas but also appear as themes running through other disciplines. The presentation in this report reflects the role of fluids and transport in the support of other microgravity disciplines.

Most physicochemical transport phenomena are influenced significantly by gravity. As a consequence, unusual behavior is expected in low-gravity environments for many fluid configurations. Also, the reduction of gravitational forces leads to dominance by other forces normally obscured in terrestrial environments, such as surface tension and electrohydrodynamic effects. Basic research is required to understand and describe the unusual characteristics of transport phenomena under low-gravity conditions.

Transport phenomena also play essential roles in many processes that are important to mission-enabling technologies. Predictive models for low-gravity performance and operation of those technologies are frequently inadequate. New models are needed. Strictly empirical approaches are not preferred for low-gravity applications because they are costly and time consuming, and can result in products or systems that are unreliable or inefficient.

Priority should be given to the study of phenomena that are prominent in the low-gravity environment and to those that are critical to space mission-enabling technologies and commercial developments. Among the basic topics that may be studied uniquely with special advantages in the low-gravity environment are the following:

- *Surface tension gradient-driven flows and capillary effects.* These are frequently obscured in a terrestrial environment but may become significant or dominant in reduced gravity. These topics warrant investigation because they are not well understood and because surface tension-driven flows are ubiquitous for many spaceflight-enabling technologies.
- *Multiphase flows.* Many processes involve multiphase flows. Gravity imposes a specific orientation on multiphase fluids and structures (e.g., gas-liquid, liquid-solid). In a reduced-gravity environment, multiphase flows and associated transport phenomena become significantly different because of the altered orientation of the various phases.
- *Diffusive transport processes.* At 1 g, multicomponent fluids experience various modes of thermosolutal convection. In addition, there are other effects due to different diffusivities for heat and mass. With reduced-buoyancy convection, these complex interactions can be separated and analyzed. Furthermore, transport effects masked at 1 g, such as Soret and Dufour phenomena, can become important.
- *Colloidal phenomena.* At 1 g, the nature of surface and short-range forces, and their consequences in colloidal systems, are often difficult to study because of the complications associated with competing gravitational effects. Microgravity provides an opportunity for study of colloidal systems in which short-range forces are dominant, which can contribute in an important way to the understanding of these physicochemical interactions.

The above topics appear prominently in many areas of science including combustion, biotechnology, materials science, and physics. Therefore, fluid mechanics and transport phenomena should be viewed as a common theme throughout many of the microgravity disciplines.

The following topics deserve attention for both their intrinsic scientific importance and their applications to space technologies:

- *Convective processes at low Reynolds number.* Investigation of low Reynolds number flows with density variations, at reduced-gravity levels, will reveal the altered nature of transport phenomena in a new range of parametric conditions.
- *Transport processes with a phase transition.* The modified processes of condensation, evaporation, and boiling in a low-gravity environment with dominant interfacial forces require study.
- *Complex materials.* Porous, granular, colloidal media, and foams are complex materials whose structures and functions can differ in microgravity. Research is necessary to understand the behavior of such materials in a low-gravity environment and may also lead to a better understanding of their behavior at 1 g.
- *Materials processing.* Buoyancy, sedimentation, and interfacial phenomena influence such processing methods as fluidized-bed hydrogenation, electrowinning, and vapor-phase pyrolysis and therefore should be investigated.
- *Physical processes in life- and operating-support systems* (enabling technologies). Some of the effects indicated above apply to processes such as power generation and storage, water purification, oxygen production, and fuel and fluid storage and management.

Numerous parameters govern the topics listed above. Judicious choice of the parameter ranges and configurations will be required to ensure that the information obtained is directly applicable to the reduced-gravity environment.

Clearly, fluids and transport phenomena appear as critical technical issues in many spaceflight-enabling technologies.

Combustion

Combustion involves fluid mechanics, mass and heat transport, and chemical reaction—all directly or indirectly subject to numerous gravitational effects. The number of parameters to be investigated is large. Several phenomena require long-duration observations and measurements (e.g., smoldering and flame spread).

The following research areas are recommended for emphasis according to rank-order:

1. The highest-priority area, and one of intense practical interest, is that of fires in spacecraft and potential extraterrestrial bases. Microgravity and reduced-gravity research is required because of the potential for disaster posed by fires. Much is still unknown about fires in altered gravity conditions. A variable gravity capability would be useful for a full understanding of gravitational effects and for implementation of fire safety measures in spacecraft.
2. In fire research, several subfields of combustion need to be investigated under microgravity conditions. Ignition, flammability limits, smoldering, flame spread, and extinguishment are all deserving of detailed study. Variables should include fuel type and phase, fuel:oxidizer ratio, ambient oxidizer concentration, forced and free convection, ignition source, and extinguishment methodology.
3. Turbulent combustion processes are highly important on Earth but cannot be probed at 1 g at the small size scales that typically occur. Laboratory scale-up at 1 g introduces unwanted buoyancy. Reduced gravity would allow a scale-up in overall size without the introduction of major buoyancy effects, thus permitting access to the smallest scales of turbulence that are important to the problem. Here, the Reynolds number based on a forced flow velocity would remain fixed in the scaling process. Even though gravity is not an important parameter for many practical devices, the laboratory study of combustion processes is often impeded at 1 g.
4. Research on laminar premixed and diffusion flames and spray-flow interactions should be conducted, again because of the importance of these processes on Earth. The same problem occurs here as with turbulent flames. That is, scale-up to allow study at 1 g introduces unwanted buoyancy effects.

In addition, other general recommendations are noteworthy.

- Reproducibility must be verified. Many combustion phenomena require a survey of a wide range of parameters. Also, some combustion phenomena are long-duration events. Consequently, an extended orbiting platform capability will be required for many combustion experiments for complete and serious study.
- Relevant to all areas above, ground-based experiments should be undertaken to develop miniaturized diagnostics and experimental apparatus and techniques for performing multiple repetitions of a specific experiment.

Biological Sciences and Biotechnology

The biological sciences and biotechnology are experimental disciplines that are highly dependent on empirical approaches to the solution of problems and on the continued discovery and development of useful research systems. Unlike many other branches of microgravity science such as fluid dynamics or materials science, there may be no firm foundation of theory, only a limited accumulation of experience. Thus, a reasonably high tolerance for scientific risk (which is clearly distinct from safety risk) should be allowed in investigations in the biological sciences. The science community should be prepared, however, to support research in this area on the basis of its long-term potential and importance, especially its relevance to human spaceflight. Since research on biological topics commonly requires experiments that take a long time to complete, an extended-duration orbiting capability is necessary.

Priorities should be given to scientific issues in biological sciences and biotechnology in the following order:

1. In studies to improve methods of crystallization of macromolecules for use in diffraction studies, additional experiments should focus on defining quantitatively those macromolecular crystal properties and growth mechanisms affected by gravity. There is demonstrated success in this research area and it is recommended for the highest priority.
2. Further experimentation is needed in both terrestrial and microgravity environments to develop new methods, materials, and techniques to exploit the potential of microgravity, where it exists, for improvements in biochemical separations. These separations are important both in terrestrial applications and in materials processing to support human spaceflight.
3. A dedicated effort should be made to evaluate the potential advantages of the microgravity environment for the study of cellular interactions, cell fusion, and multicellular assembly and to identify candidate cell systems that show maximum response to being cultured in such an environment. Efforts should also be made to identify and characterize subcellular mechanisms that sense and mediate responses to the magnitude and direction of gravitational forces.
4. A systematic effort should be made to identify those cellular and biomolecular processes, structures, assemblies, and mechanisms that may be affected by gravity, and to design and carry out experiments to explore the effects of microgravity on appropriate systems.

Materials Science and Processing

Metals and Alloys. The microgravity environment provided by an orbiting spacecraft offers new opportunities in the control of melting and solidification processes. Reduction of convective velocities permits, in some cases, more precise control of the temperature and composition of the melt. Body force effects such as sedimentation, hydrostatic pressure, and deformation are similarly reduced. Weak noncontacting forces derived from acoustic, electromagnetic, or electrostatic fields can be used to position specimens while they are being processed, thus avoiding contamination of reactive melts with their containers.

Since many experiments in this field require long time scales for completion, the capability of an extended-duration orbiting platform is needed to obtain full benefits from microgravity research. Topics in the metals and alloys area that would benefit from a focused effort include the following prioritized items:

1. Nucleation control and the achievement of metastable phase states, such as metallic glasses and nanostructures, are areas that could benefit from achieving deep supercooling in the microgravity environment, by elimination of container surfaces, and from the reduction of melt flows due to buoyancy-driven convection.

2. Microgravity experiments on Ostwald ripening and phase coarsening kinetics would add quantitative, fundamental information about the key metallurgical issues of interfacial dynamics during thermal and solutal transport, and the question of microstructure evolution in general.

3. Observations of aligned microstructures processed reproducibly under quiescent microgravity conditions should help to provide well-defined thermal processing limits for polyphase directional solidification of eutectics and monotectics.

4. Studies of the formation of solidification cells and dendrites under well-defined microgravity conditions can add to our expanding knowledge of complex metallurgical pattern formation and, more generally, of the fundamental physics of nonlinear dynamics. Microgravity conditions can be useful in these instances for the pursuit of sophisticated tests of theory and the quantification of metallurgical pattern dynamics.

5. Some thermophysical properties can be measured advantageously in microgravity. Accurate data on such properties, frequently essential for the modeling of metallurgical processes and materials responses, are often not available from standard terrestrial measurements.

Polymers. Polymers potentially represent the broadest classes of engineered materials, permitting great innovation and precision in design, including control at the molecular level. Although the viscous character of most high polymer melts greatly desensitizes their response to gravitational acceleration, nonetheless some polymer solutions have relatively low viscosities. Moreover, some of these organic systems provide materials with interesting applications in the fields of nonlinear optics and metrology.

- A few initial experiments on the vapor- and solution-phase processing of organic and polymer films in microgravity have shown improved texture and smoothness over terrestrial counterparts, suggesting that this area of research merits further study.

Growth of Inorganic Single Crystals. The microgravity environment will be particularly useful for the study of transport phenomena in the liquids from which bulk crystals are grown, and priority should be given to these studies rather than to the growth of large crystals. Such studies probably require a steady, very-low-gravity environment such as that obtainable in a free-flyer and will provide useful data without requiring growth of bulk crystals. Precise transport data will become particularly useful for fluid dynamics computations, which are rapidly improving for terrestrial melt and solution growth. Any experiments on bulk crystal growth must be judged by their potential to contribute to the scientific understanding of the fundamental processes of crystal growth. The recommendations for this area of research are as follows:

- The design and execution of microgravity experiments that lead to a better fundamental understanding of crystal growth have proved elusive, and the committee recommends against the growth of large inorganic crystals under low gravity. The best approach to understanding the details of such growth will likely derive from fluid dynamical modeling and the modeling of processes at the fluid-solid interface, along with terrestrial studies of crystal growth. This analytical approach may provide the rationale for the growth of benchmark-quality inorganic crystals in microgravity.

- Assuming that some of the versatility of terrestrial experimentation can be achieved in the microgravity environment, there are opportunities for microgravity research that will have an impact on terrestrial bulk crystal growth. Priority should be given to transport studies, including studies of solute and self-diffusion, heat diffusion, and Soret diffusion. All of these involve the fluid from which crystals are grown, and they are amenable to routine study with repeatedly used apparatus. However, they may require a more stable environment than that provided by the space laboratory. Indeed, since such studies will undoubtedly be sensitive to the acceleration environment, they may also be useful for the study of this environment as a variable in the low-gravity range.

- Precise transport data will become particularly useful since fluid dynamical computational capabilities (for which these data are required) are improving rapidly for terrestrial melt and solution growth, as well as other industrial processes. Furthermore, transport measurements in industrially important fluids may be an important microgravity application outside the realm of large inorganic crystal growth.

Growth of Epitaxial Layers on Single-Crystal Substrates. During the terrestrial growth of epitaxial layers by vapor deposition, an effort is made to minimize the effects of flow, buoyancy, and boundary layer uniformity by rotating

or spinning substrates during the growth process. In addition, attempts are under way to calculate precise flow patterns and resulting growth rates in model systems. The calculations are complex, and it is uncertain whether they will be able to predict occurrences in real systems that are useful for industrial production. Since the manufacture of heterostructures by vapor deposition methods will be accomplished terrestrially, it is not clear whether any relevant data can be obtained solely under microgravity.

- A low priority is recommended for chemical vapor deposition studies under microgravity conditions.

For molecular beam epitaxy (MBE) methods, great gains in purity can, if necessary, be made on Earth, if sufficient attention is given to reducing contamination and increasing pumping speed. The committee concludes that epitaxial layers will be too costly to manufacture in space and that, at a reasonable cost, much improvement in the vacuum environment can be achieved terrestrially.

- Molecular beam epitaxy studies in orbiting vehicles are not appropriate until the limits of ground-based alternatives have been clearly reached.

Ceramics and Glasses. As discussed above, a low-gravity environment will reduce buoyancy-driven convective flows in liquids. Most ceramic synthesis and processing is done at high temperature either by solid-state processes exclusively or by processes in which there are only small amounts of viscous liquid phases. Glasses are formed from high-temperature melts, where the suppression of convective mixing is generally undesirable because convection promotes compositional homogeneity. A second case in which liquids are important to ceramics is in the synthesis of ceramic powders or films from aqueous solutions or colloidal suspensions. Frequently, the requirement is to achieve a high density of nuclei and consequent fine particle sizes of the precipitated powders. Thus, rapid mixing is used, and there is no reason to suppress convection. For these reasons, low-gravity studies are of limited advantage in this discipline.

Nonetheless, a low-gravity environment might be of benefit to ceramics research and development in the following prioritized areas:

1. There is interesting potential for containerless melting. Processing of ceramics at high temperature requires refractory containers that remain unreactive with the specimen. Availability of a general capability for containerless, high-temperature processing (to at least 1600°C) would allow contamination-free synthesis of glasses and ceramics, such as those being considered for optoelectronic applications, as well as the study of glass nucleation and crystal growth without heterogeneous nucleation on a container wall.
2. A fundamental study of crystal nucleation and growth in glass melts in microgravity would be interesting. Crystallization is an important process, desirable in glass-ceramics and undesirable in optical glasses, that requires further characterization and understanding.
3. Mass transport and diffusion studies of glass and ceramic melts under microgravity conditions should generate more precise data than those available from terrestrial measurements. One area of research that might be aided by reduction of convection is obtaining accurate data on diffusion in ceramic melts.
4. The suppression of free evaporation from melt surfaces could allow synthesis at higher temperatures than can be performed on Earth.
5. The epitaxial growth of films from solution, including biomimetic synthesis (self-assembling monolayers) of ceramics, should be studied.

Microgravity Physics

Experiments in this area, with the exception of the general relativity test Gravity Probe-B (GPB), all represent extensions of work that is, or can be, conducted in Earth-based laboratories. Much of the scientific value of the proposed space experiments will depend on the strength of the connection to Earth-bound research. Given the long time scale for the ground-based development through flight of a space experiment, there is concern that the scientific goals of the experiment might be bypassed by new developments or by major shifts in the value ascribed to the work.

The topics of microgravity physics fall into two general categories: (1) development of new instruments (e.g., superconducting gyroscopes for the GPB experiment and the mass balance for the equivalence principle test), and (2) preparation and study of unique samples such as uniform fluid free from gravity-induced density gradients or low-density granular materials near a percolation limit. This program can include such important studies as fundamental physics measurements (e.g., verification of the equivalence principle), critical phenomena, dynamics of crystal growth, and low-density aggregate structures.

A recent successful experiment in this area is the Lambda Point Experiment (LPE) that was flown on the shuttle in October 1992 (USMP-1). Analysis of the data indicates an improvement of nearly two orders of magnitude over previous data obtained on Earth. This has been done in an unbiased way, and heat capacity data have been obtained approaching a few nanokelvin of the Lambda point. This experiment demonstrates clearly that highly sophisticated experiments involving the most sensitive and advanced instrumentation can be profitably performed in the microgravity environment.

The quality of the microgravity environment must be examined critically in the context of each possible experiment. Minimum acceleration is the most obvious parameter of concern for many of the contemplated experiments; however, the time span over which a high-quality, low-gravity environment can be maintained may be of equal importance. For some experiments, accidental large accelerations (perhaps resulting from sudden movements of personnel or the firing of small thrusters) might destroy the object of study, for example, a low-density granular structure.

With regard to the quality of the microgravity environment available for experiment, only the center of the orbiting spacecraft is in true free-fall and then only to the extent that orbital drag effects and other external influences are negligible. In a gravity gradient-stabilized spacecraft, there will be a steady rotation of any experiment about the center of mass of the entire spacecraft once each orbit. For a low Earth orbit, this will result in accelerations on the level of 10^{-7} -g level at a distance 1 meter from the center of mass of the entire orbiting system. On both the space shuttle and the space station, only a few experiments will be located close enough to the center of mass to ensure acceleration levels below the 10^{-6} -g level.

- A number of scientifically meritorious projects, such as the equivalence principle experiment and GPB, will require spaceflight independent of any manned space facilities.

In the future, we may anticipate a continued requirement for low-temperature facilities in space, since low temperatures are important for the highest-resolution measurement techniques, particularly those based on SQUID (Superconducting Quantum Interference Device) technology.

- If a space station is to be a useful contributor to the area of fundamental science, access to liquid-helium facilities will be mandatory.

GENERAL SCIENTIFIC RECOMMENDATIONS FOR MICROGRAVITY RESEARCH

Following are a number of general science recommendations that are important for microgravity research:

- The reproducibility of results is a crucial element of laboratory science, and flight investigators should be given the opportunity to address reproducibility in their research. Nonetheless, a balance should be established between the reflight opportunities necessary for reproducibility and the flight of experiments that address new scientific issues.
- The need for scientific judgment, trained observation, and manned intervention and participation in certain laboratory microgravity experiments requires a greater use of payload specialists with expertise and laboratory skills directly related to the ongoing experiments. Some flight experiments would also benefit from the direct investigator interaction made possible by teleoperation capabilities, and NASA should support the development and deployment of such techniques in future microgravity experiments.
- As a rule, microgravity experiments should be designed and conducted to provide specific explanations for meaningful scientific questions. Scientific objectives should be clear and specific. In addition, NASA must

continue to support ground-based experiments and the development of underlying theories. Theoretical understanding and ground-based experimental results should support the need for microgravity experiments and the likelihood that experimental objectives will be met. Notwithstanding this, exploratory experiments can be valuable in advancing understanding of some complex systems (e.g., biological systems).

- There are scientific reasons to augment microgravity research in certain areas with a variable-gravity capability of extended duration that covers the microgravity to 1-g range. In a laboratory science where controlled model experimental systems are essential, the ability to vary an important parameter continuously should be developed whenever feasible. Therefore, if a space station centrifuge is to be developed, it is desirable to evaluate shared utilization for microgravity research.

- NASA should categorize experiments according to their minimum facility requirements to maximize scientific return and cost-effectiveness. Drop towers, aircraft on parabolic trajectories, sounding rockets, and orbiting platforms supply a range of acceleration levels, acceleration spectra, and experimental durations and provide opportunities for manned interaction and demonstration of reproducibility. Some facilities are more suitable than others for precursor experiments to evaluate instruments and procedures and to demonstrate feasibility.

- General-purpose facilities (versus experiment-specific equipment) should not be imposed on principal investigators if it might degrade the scientific results. Costs are not necessarily lowered when equipment is designed for a wide range of experiments. The scientific benefits could be substantially reduced by the inherent compromises.

- For each flight experiment, the acceleration vector should be accurately measured locally, frequently (or continually), and simultaneously with other experimental measurements. Since the magnitude and direction of the net local acceleration environment can significantly affect experiments, they must be correlated with the primary experimental data. The net acceleration vector varies temporally and with position relative to the spacecraft center and can have major effects even at small magnitudes.

- Materials studied in microgravity experiments should be adequately characterized on Earth. For some materials, there is a lack of the thermophysical data essential for both experimental design and modeling. These data should be obtained by ground-based research, where possible, or from microgravity measurements, when necessary.

- The qualitative effects of the acceleration environment on certain types of experiments should be studied. These effects are not generally understood and characterized.

FLIGHT OPPORTUNITIES AND CHALLENGES

The peculiar character of microgravity research as a laboratory science in space requires real-time interactions between scientists and their experiments. This special requirement indicates another role for humans in space, in addition to the traditional use of astronauts for exploration and the deployment and repair of satellites. Moreover, a laboratory-based researcher should be able to carry out a large number of experiments to cover required ranges of the experimental parameters and demonstrate the reproducibility of results. The researcher should be able to modify the apparatus and diagnostics, when necessary, to ensure successful experimentation.

An assortment of facilities is available for research in a low-gravity environment. These range from drop towers and aircraft to orbiting platforms. The duration required for experiments in some of the subdisciplines of microgravity research may exceed the flight time available on some platforms. Additional spacelab missions, particularly those with extended-duration capability, might adequately serve those subdisciplines. For other subdisciplines such as some aspects of biotechnology and materials science, however, longer flight times would provide significant benefits in terms of the quality of scientific yields. Even for those subdisciplines with shorter-duration experiments, the limited number of spacelab flights and the high costs associated with them severely limit the experimenter with regard to demonstration of reproducibility, time for readjusting or repairing equipment, and reflight opportunities. These limitations, inherent to the spacelab system, can be obviated only by the longer flight durations available on a space station or a recoverable satellite. It is important, however, that the design of a space station or recoverable satellite provide a stable microgravity environment that minimizes unacceptable disturbances.

The present microgravity research infrastructure does not readily accommodate the needs of laboratory research. Although drop towers and airplanes flying parabolic trajectories can be used for special or precursor experiments and some materials processing can be done on free-flyers, the spacelab and space station are better suited for microgravity laboratory research. Experience with spacelab for microgravity research, however,

indicates that (1) it is likely to take years to develop an experiment that can yield high-quality scientific data and (2) it is an extremely expensive process.

The time required from the selection of the principal investigator to launch with new hardware is 5 to 6 years for mid-deck (noninteractive) experiments and 6 to 8 years in spacelab. Reflights with minimal modifications to the equipment require 1 year for mid-deck and 2 years for spacelab experiments. Minute elements of an experiment must be documented in detail and subjected to safety tests. Experiments are often delivered up to a full year before launch for integration into the spacelab and then into the orbiter. Because the number of spacelab missions is limited, as many experiments as possible are scheduled for each flight. Thus, the number of experimental runs is limited even if all goes as planned on the mission. As a result, compromises are made that might be favorable for one experiment but not others. For those experiments that suffer in the compromise process, essential science might be sacrificed. Also, because of heavy demand, experimenters are often guaranteed only one flight.

Surmounting the various administrative hurdles confronting an experiment requires interaction with three different NASA centers. The conditions imposed on the experiment by various centers are not always mutually consistent. The principal investigator is often excluded from the discussions. Most of this complexity results from concern for the safety of the crew and spacecraft, and results in long times for experiment development and flight and, consequently, increased cost. Concern for safety in human spaceflight missions should always remain a primary consideration, but safety issues could be fully addressed in a streamlined administrative process that does not compromise the scientific objectives of the mission. The requirement for laboratory science is that human participation in the experimental process be guaranteed, although in some cases, unmanned flights with remote control of experiments are effective substitutes for physical human presence. Human presence, however, will be necessary for many spaceflight experiments in the foreseeable future.

The entire infrastructure and extensive procedures that are currently extant have been developed essentially for missions in space with purposes other than laboratory science. They should now be reassessed and perhaps modified extensively to take full advantage of the unique microgravity environment.

The biotechnology program has been affected by some confusion concerning its administrative oversight. Biotechnology is tied strongly to other microgravity programs because it shares fluids and transport phenomena as the common scientific theme through which gravity becomes an important parameter. The administrative issue concerns the need for cooperation and coordination between the microgravity research administration and the life sciences administration for research on cellular and subcellular processes and mechanisms.

In summary, the major administrative challenges before NASA in the microgravity research domain are the following: (1) interactions among centers and between centers and headquarters should be simplified and unified; (2) principal investigators should be continually involved with the development of experiments; and (3) biotechnological research and life sciences programs should be well coordinated.

ADMINISTRATIVE RECOMMENDATIONS

The following recommendations do not directly address the scientific issues, but rather summarize administrative issues that profoundly affect the quality and quantity of the scientific content of the microgravity program.

- Meaningful interaction should be maintained between the principal investigators and NASA staff during experiment development and integration, and communication among these groups must be continued following flight. It is essential to minimize the overlap of responsibilities in the NASA infrastructure to accomplish this goal.
- It is essential that the overlap of responsibilities among NASA centers and between centers and headquarters be substantially reduced in order to optimize the influence of the principal investigators and the likelihood of successful experiments.
- Measures should be taken to improve coordination and cooperation between the life sciences administration and the microgravity research administration concerning biological and biotechnological research on cellular and subcellular processes and mechanisms. The strong scientific coupling between biotechnology and other microgravity disciplines through the fluids and transport theme should be recognized in any administrative reorganization.
- All data, including acceleration measurements, should be made available immediately to the principal investigator. Current delays in providing data extend many months beyond the flights. NASA should coordinate the operations of various offices so that priority is given to the processing of experimental data.

- It is necessary to increase substantially the number of ground-based investigations to ensure the future supply of high-quality flight experiments. Consequently, the budget for ground-based research should be increased as a fraction of the entire microgravity program. To the maximum extent feasible, funding for ground-based research in the microgravity program should be protected from temporary budgetary fluctuations.
- In addition to its role in developing flight experiments, the ground-based program can provide important scientific and technical data for other purposes. A ground-based study can therefore be judged successful even if a flight experiment does not result.
- Microgravity research is much broader than the topic of materials processing in space and should be identified as *microgravity research* in all official documents, including the federal budget. Microgravity research better describes the activity and its actual and potential accomplishments. Materials processing in space characterizes only a fraction of this research activity and promotes a misleading impression of the potential benefits and scope of the program.
- The microgravity sciences and applications program and the commercial development program have a large area of overlap and common interest. Coordination between these programs and an equally stringent review process for each should be fostered by NASA for their mutual benefit.
- NASA should establish mechanisms for continuous, unrestricted submission of research proposals in order to optimize quality and take advantage of scientific advances. Unrestricted submission of research proposals would parallel approaches of other funding agencies and enable continual scientific advance.
- To manage the diverse disciplines in microgravity research, it is necessary to increase the breadth and experience of the scientific staff at NASA headquarters. A rotation of prominent scientists on leave from universities, national laboratories, and industry is one mechanism to be explored. Introduction of active scientists in the administration of the program would be highly beneficial.
- Prompt documentation of experimental results should be required and enforced. Reports of all experiments, including unsuccessful efforts, should be accessible to all interested parties. A concern is that some investigators might not report results because they are proprietary or inconclusive. The lack of an available report could lead to unnecessary duplication of efforts.
- NASA should organize and maintain an accessible archive of microgravity research results. This archive should contain a bibliography of all published scientific papers and reports on microgravity subjects and should preserve the original spaceflight data sets, such as photographs and electronically recorded data.

An additional point of concern is that given the long time scale for the development through flight of a space experiment, there is a real danger that the scientific goals of the experiment might be bypassed by new developments or by major shifts in the value ascribed to the work. There is also the possibility that the principal investigator may lose contact with the field. Several of the above recommendations may be useful in this regard. Anything that NASA can do to shorten this time frame would be beneficial.

3.3 Review of Gravity Probe B

A Report of the Task Group on Gravity Probe B¹

SUMMARY

BACKGROUND

The experiment now known as Gravity Probe B (GP-B) was conceived more than 30 years ago. Bold and daring in concept, it has been under continuous development ever since. The aim of the experiment is to measure, rather precisely, an effect that is predicted by all viable relativistic theories of gravity but has not yet been observed. Just as Newton's law of gravity is paralleled by Coulomb's law of electricity, so also it is expected that the force between currents of electrical charge, described by Ampere's law, should be paralleled by a force between "currents" of flowing matter. It is this force that has never been directly observed.

A useful perspective on the GP-B experiment can be obtained from a historical profile of its funding. Until the late 1980s, the project was funded at a level of \$1 M to \$2 M per year to develop and demonstrate the necessary technology. Funding was then increased to permit detailed engineering of the various subsystems and thorough ground testing. The funding level reached about \$30 M/yr in FY 1992, when the project entered a "science mission" phase involving development of an appropriate spacecraft to carry the experiment. Since then the funding has been approximately \$50 M/yr.

When the project was last reviewed for NASA 4 years ago, the Parker Committee, an ad hoc review committee convened by NASA Associate Administrator for Space Science and Applications Lennard A. Fisk and chaired by Eugene N. Parker of the University of Chicago, recommended that if GP-B were to go forward, it must be properly funded. That committee considered an appropriate funding level to be about \$50 M/yr until the time of launch, which was anticipated to be late in the 1990s. Subsequent funding has in fact been at this level, and has allowed highly skilled teams to address thoroughly various technical details of the experiment and to start building the flight instrument package and integrating it into a spacecraft. By the end of FY 1995 about \$240 M will have been spent on the project. NASA estimates that another \$340 M will be needed for completion, including launch and subsequent data analysis.

SCIENTIFIC MOTIVATION

Like most other fields of science, Einstein's theory of gravity, the general theory of relativity or GR, has developed its own notation and jargon. Despite the simplicity and economy of its underlying assumptions, the theory in full glory leads to intensely complicated nonlinear equations. Indeed, the equations have been fully solved only in a few special instances. However, much of the mathematical complication can be removed by assuming that all gravitational fields are weak. The equations then reduce to a form remarkably similar to those governing electromagnetism. Terms appear that are analogous to the electric field caused by charges (the *gravitoelectric field*, produced by masses), and to the magnetic field produced by the flow of charge (the *gravitomagnetic field*, produced by the flow of matter). A spinning ball of electrical charge produces a well-prescribed static magnetic field, and correspondingly a spinning mass such as the Earth is expected to produce a static gravitomagnetic field. Of course, general relativity has important differences from electromagnetism, as well: in particular, it represents gravitational forces as arising from geometric curvature in the structure of space and time.

Gravity Probe B aspires to detect and measure, at the 1 percent level, the gravitomagnetic field produced by the spinning Earth through a spin-spin interaction with an orbiting gyroscope. This effect of the gravitomagnetic field is often referred to as "frame dragging," or the Lense-Thirring effect. In addition, GP-B will accurately measure the much larger "geodetic" precession, a combination of the effects of spin-orbit coupling and space-time curvature.

In the quarter century since inception of the GP-B project, many other tests of Einstein's theory of gravity have been made. The delay and deflection of light signals passing close to massive objects have been measured with increasing precision and found to agree with the predictions of GR at the 0.1 percent level. Geodetic precession has

¹"Summary" reprinted from *Review of Gravity Probe B*, National Academy Press, Washington, D.C., 1995, pp. 1-4.

been detected and measured with 2 percent accuracy by laser ranging to the Moon. Gravitational radiation from accelerated masses in a binary pulsar system has been shown to be consistent with GR at the 0.4 percent level. Some of these tests involve gravitomagnetic effects related to the translational flow of matter, in combination with other relativistic gravitational effects, and therefore they provide indirect evidence for the existence of gravitomagnetism. By contrast, GP-B proposes to provide a direct test of gravitomagnetism caused by rotation, in isolation from other relativistic gravitational effects.

The past quarter century has also seen the development of exquisitely sensitive new instruments based on developing technologies and located both on Earth and in space. Some of them have provided the means to probe more and more deeply into the nature and evolutionary history of the universe. Observations with such instruments have yielded one surprise after another, and they raise perplexing questions about missing mass, the age of the universe, and the circumstances giving rise to the large-scale distribution of matter in space. In the past, laws of nature previously considered sacrosanct have sometimes been found deficient when subjected to much closer scrutiny or applied to new phenomena. As long as some discoveries defy understanding, it is important to continue testing nature's most fundamental laws.

CONCLUSIONS

Scientific Importance

The frame-dragging effect predicted by our principal theory of space and time, general relativity, has a deep conceptual significance involving the connections between rotation, distant matter, and absolute space. Frame dragging is a direct manifestation of gravitomagnetism. Its consequences have found important astrophysical applications in, for example, models of relativistic jets observed streaming from the cores of quasars and active galactic nuclei. A 1 percent measurement of the predicted frame-dragging effect would be a significant and unique test of GR. Gravity Probe B is one of the few space missions NASA has conducted with relevance to fundamental physics. If successful, it would assuredly join the ranks of the classical experiments of physics. By the same token, a confirmed result in disagreement with GR would be revolutionary.

Since GP-B was conceived, significant progress has been made through experimental studies of gravity, both in improved precision and in performing qualitatively new tests. These tests are so constraining that there are now no examples of alternative theories that are consistent with the experimental facts and predict a frame-dragging effect different from that predicted by GR at a level GP-B could detect. Yet the basic weakness of the gravitational force means that GR has been tested much less thoroughly than the other fundamental theories of physics. Nevertheless, along with most physicists this task group believes that a deviation from GR's prediction for frame dragging is highly unlikely.

In addition to detecting the new gravitomagnetic effect of frame dragging, Gravity Probe B should be able to measure the geodetic precession of its gyroscopes to an unprecedented accuracy of about 75 parts per million (ppm). This result would provide a factor-of-20 improvement in the measurement of space curvature per unit mass (now known to about 2 parts in 1000) and would tightly constrain the deviations from GR predicted by other theories of gravity in the weak-field limit.

Technical Feasibility

The task group is highly impressed with the extraordinary talents and abilities of the technical team assembled to create Gravity Probe B. The group has consistently solved technical problems with great inventiveness and ingenuity. Moreover, in the course of its design work on GP-B the team has made brilliant and original contributions to basic physics and technology. Its members were among the first to measure the London moment of a spinning superconductor, the first to exploit the superconducting bag method for excluding magnetic flux, and the first to use a "porous plug" for confining superfluid helium without pressure buildup. They invented and proved the concept of a drag-free satellite, and most recently some members of the group have pioneered differential use of the Global Positioning System (GPS) to create a highly reliable and precise aircraft landing system.

The task group finds progress in construction of the actual GP-B apparatus to be very impressive, as well. Working in concert with a team from the Lockheed Missiles and Space Company, the Stanford group is well on its way toward putting GP-B into space before the end of the decade, providing that the funding level is sustained. The

task group has found no serious technical impediments to meeting the existing launch schedule. The spacecraft, experimental package, and projected methods of operation are well designed to meet the scientific requirements and prove the results valid. The team is well prepared to cope with a wide range of unanticipated phenomena. The task group considers the overall complexity of GP-B to be somewhat greater than that of the Cosmic Background Explorer (COBE) but much less than that of the Hubble Space Telescope (HST). An ordinary hardware failure is no more likely than in other comparable space missions. Furthermore, GP-B has been designed with extensive in-flight testing of all parts, four independent sensor gyros to provide immediate confirmation of results, and in-flight calibration using observations of the aberration of light caused by the motion of the satellite.

Nevertheless, the extraordinary experimental requirements and the impossibility of ground tests of some critical systems at the necessary level of accuracy introduce significant risks. Despite an extensive list of detailed questions put to the GP-B team by the task group, no specific weakness or likely points of failure have been identified. A majority of the task group believes that GP-B has a reasonably high probability of achieving its design goals and completing the planned measurements. However, based on their experience with complex scientific experiments on the ground, several members remain skeptical about the large extrapolations required from ground testing to performance in space. This minority believes it likely that some as yet unknown disturbance may prevent GP-B from performing as required. The task group notes that in any event, should the GP-B experiment be completed successfully but yield results different from those predicted by general relativity, the scientific world would almost certainly not be prepared to accept them until confirmed by a repeat mission using GP-B backup hardware, or by a new mission using different technology.

Comparison with Other Proposed Programs

The scientific objectives of GP-B involve testing one of the fundamental laws of nature. The goals are therefore quite different from the objectives of a common situation in which natural laws, as inferred theoretically and tested in terrestrial laboratories, are used to interpret observations of astrophysical phenomena. In particular, the ambitions of GP-B are qualitatively different from those underlying most astronomical work, including NASA projects such as the HST, the Stratospheric Observatory for Infrared Astronomy (SOFIA), the Space Infrared Telescope Facility (SIRTF), and the Advanced X-ray Astrophysics Facility (AXAF). Tests of nature's laws are the ultimate foundation of physical science and are the only rational basis for belief that these laws are, at least in part, "understood." Despite its omnipresence, gravity remains the least well tested of all the fundamental forces.

NASA's highly successful COBE satellite was designed primarily to answer certain astrophysical and cosmological questions. Nevertheless, its results have implications in fundamental physics as well, particularly for questions concerning the origin of the universe. The task group's considered judgment is that the most likely of successful outcomes of the GP-B experiment—the measurement and confirmation of two specific effects predicted by general relativity—will be an important milestone, but will have less impact on the scientific world than the cumulative results of COBE. The reason is simple: there is no serious alternative to the general theory of relativity that predicts effects differing from those of general relativity by amounts that GP-B could detect. The GP-B experiment has been exciting for many scientists because of the need for confirmation of gravitomagnetism and the possibility of a great surprise, but the latter chance now seems more remote than before.

Other proposed satellite tests of frame dragging or spatial curvature, such as LAGEOS III, are intrinsically an order of magnitude less precise than GP-B. Another proposal claiming to offer higher accuracy is now in the conceptual stage and might eventually become a worthy successor to GP-B. It is discussed briefly in the section "Other Tests of Frame Dragging or Geodetic Precession" [in Chapter 2].

NASA estimates that \$340 M will be required to complete the construction, launch, and data analysis phases of GP-B. If the experiment delivers as promised, so that the frame-dragging effect is measured to 1 percent accuracy and the geodetic term to 75 ppm, is it worth the cost? This question must be viewed in the context of other NASA projects of comparable magnitude, and necessarily its answer involves subjective scientific judgments. The task group was not able to achieve a clear consensus on the question of competitive value, even after extensive discussion and deliberation. Its members agree unanimously that all scientists would find it appealing to see a clean and direct demonstration of the frame-dragging effect, and that a confirmed discrepancy between the result of the GP-B experiment and the prediction of general relativity would fully justify the mission's cost, including the additional expense of a confirming experiment. However, in light of existing tests of gravitation theories such a discrepancy is considered highly unlikely.

Consequently, the task group's members hold a range of opinions on the relative cost-effectiveness of GP-B. A significant minority judge that the purpose of the mission is too narrow in comparison with missions that explore wide-open scientific issues and have a high probability of making new discoveries. This minority assigns high weight to the fact that essentially all experts believe that gravitomagnetism must exist, and consequently it does not appear likely that unexpected new knowledge will be gained.

In contrast, the task group's majority judgment gives higher weight to the importance of experimental verification in GP-B's unique and direct test of general relativity. Considering also the possibility of a revolutionary discovery, however remote, the majority judges the GP-B project well worth its remaining cost to completion.

3.4 Earth Observations from Space: History, Promise, and Reality

A Report of the Committee on Earth Studies¹

EXECUTIVE SUMMARY

The ability to make earth observations from space is one of the great successes of the space age. For both scientists and operational users of the data, however, this success has been tempered with disappointment. The promise of the technology has not yet been realized, nor is it evident that current activities are leading toward a timely realization of that promise. The civil earth observations programs of the National Aeronautics and Space Administration (NASA) have been in a state of annual redesign for more than 5 years. The early momentum that led to the laudable concept of NASA's Mission to Planet Earth (MTPE) is being dissipated.

In this report the Committee on Earth Studies (CES), a standing committee of the Space Studies Board (SSB) within the National Research Council (NRC), reviews the recent history (nominally from 1981 to 1995) of the U.S. earth observations programs that serve civilian needs. The principal observations programs examined are those of NASA and the National Oceanic and Atmospheric Administration (NOAA). The Air Force's Defense Meteorological Satellite Program (DMSP) is discussed, but only from the perspective of its relationship to civil needs and the planned merger with the NOAA polar-orbiting system.

The report also reviews the interfaces between the earth observations satellite programs and the major national and international environmental monitoring and research programs. The monitoring and research programs discussed are the U.S. Global Change Research Program (USGCRP), the International Geosphere-Biosphere Program (IGBP), the World Climate Research Program (WCRP), related international scientific campaigns, and operational programs for the sharing and application of environmental data.

It is not the intent of the CES to make detailed reviews of every aspect of this broad scope of activities, nor is it the intent to provide detailed findings and recommendations for action by responsible agencies. Instead, the purpose of this report is to provide a broad historical review and commentary based on the views of the CES members, with particular emphasis on tracing the lengthy record of advisory committee recommendations. Any individual topic could be the subject of an extended report in its own right. Indeed, extensive further reviews are already under way to that end. If the CES has succeeded in the task it has undertaken, this report will serve as a useful starting point for any such more intensive study.

The report is divided into eight chapters: (1) an introduction, (2) the evolution of the MTPE, (3) its relationship to the USGCRP, (4) applications of earth observations data, (5) the role that smaller satellites can play in research and operational remote sensing, (6) earth system modeling and information systems, (7) a number of associated activities that contribute to the MTPE and the USGCRP, and (8) organizational issues in the conduct of civil earth observations programs. Following the body of the report is a series of appendixes: after a list of acronyms and abbreviations and collected short biographies of CES members, six brief tutorials discuss several scientific topics important to the science and applications of earth observations. Highlights from the eight chapters follow.

EARTH SCIENCE FROM SPACE AND THE EVOLUTION OF THE MISSION TO PLANET EARTH

The NASA effort in earth observations is called the Mission to Planet Earth. It includes (1) a number of intermediate-size satellites that are collectively called the Earth Observing System (EOS); (2) a series of smaller satellites called Earth Probes; (3) a major information system named the EOS Data and Information System (EOSDIS); (4) associated research, data analysis, and mission operations activities; and (5) the Landsat-7 satellite, which will be the joint responsibility of NASA and NOAA. In addition, the MTPE relies on the availability of data from NOAA's operational satellites, the DMSP satellites (up to the point of their merger with the NOAA polar-orbiting satellites), and numerous foreign satellites—some wholly foreign owned, others carrying NOAA or NASA instruments in cooperative ventures.

¹"Executive Summary" reprinted from *Earth Observations from Space: History, Promise, and Reality*, National Academy Press, Washington, D.C., 1995, pp. 3-10.

The Mission to Planet Earth began as a scientific initiative called Global Habitability, which had its origins in the research and operational earth observations missions of the 1960s and 1970s. The Global Habitability effort culminated in the report of a NASA workshop chaired by Richard Goody entitled *Global Change: Impacts on Habitability* (JPL, 1982). The next step in the advancement of the idea came in the report of an NRC workshop chaired by Herbert Friedman entitled *Toward an International Geosphere-Biosphere Program* (NRC, 1983). The concept continued to evolve and took much of its present form from the seminal work of the Earth System Science Committee (ESSC), which was chaired by Francis Bretherton (NASA, 1986, 1988). These three pivotal efforts became known colloquially as the Goody, Friedman, and Bretherton reports.

The MTPE was allied early with the Space Station program. As a result of that tie, the program took the initial form of a satellite design using very large, highly complex, astronaut-tended, polar platforms that would be serviced from the Space Shuttle, which was required to fly in a polar orbit for this purpose. After planning for launches of the Space Shuttle into polar orbit and astronaut servicing of platforms there was eliminated, the separation of the MTPE from the Space Station program soon followed. A continuous series of reviews, redesigns, budget reductions, and changes in scope has led to the current configuration that employs intermediate and smaller satellites. Reviews are ongoing in 1995, with Congress calling for a reexamination of the program by NASA, the General Accounting Office (GAO), the Marshall Institute, and the NRC.

The continuing redesign has led to (1) large amounts of discarded work, (2) a reduction in the scientific and technical scope of the effort, (3) a retreating series of dates for the achievement of the originally stated scientific and technical goals, and (4) a continuous distraction for the scientists and administrators attempting to carry out the program. It is the CES's perception that a substantial fraction of the overall effort conducted over a period of 5 years, particularly that by people within NASA, has been devoted to responding to calls for changes to the program from the Administration, the agency, and Congress.

Approximately 6 years prior to the present report, a blue-ribbon panel was formed to examine the future of the U.S. civil space program; it was chaired by one of the nation's most prominent industrial leaders. The panel's report noted (Augustine, 1990),

"Management turbulence" [is] defined as continual changes in cost, schedule, goals, etc. . . . Each change induced has a way of cascading through the entire project execution system, producing havoc at every step along the way. . . . At each step, contracts must be renegotiated, people reassigned, designs changed and schedules revised. Soon a disproportionate amount of time is spent in the pursuit of these change practices instead of producing the end product itself.

The report also noted,

The impact of excessive revisions in research contracts conducted by universities has much the same effect. In this case, substantial effort is devoted by academic researchers to the preparation of proposals for research support. When the presumed funds to support the work are subsequently diverted to other objectives, the productive talents of some of the nation's most able people are largely wasted.

The CES finds the Augustine panel's language to be an accurate and troubling description of what has overtaken the MTPE.

MISSION TO PLANET EARTH AND THE U.S. GLOBAL CHANGE RESEARCH PROGRAM

The U.S. Global Change Research Program (USGCRP) was created in response to public concerns regarding environmental change and stemmed from earlier national and international programs (e.g., the WCRP, United Nations Environmental Program (UNEP), IGBP, and NASA's work leading up to the MTPE). An early characteristic of the USGCRP was the tendency to include under its umbrella nearly all preexisting environmental programs within federal agencies, whether or not they were part of an organized, coherent program of research on global change.

In the context of this report, *climate change* refers to changes on time scales from a few years to a few centuries in the climate. Following Lorenz (1975) and Peixoto and Oort (1992), *climate* is defined as "the mean physical state of the climate system . . . , consisting of the atmosphere, oceans, and cryosphere. The 'mean physical state' is

defined as a set of averaged quantities complete with higher moment statistics . . . that characterize the structure and behavior of the atmosphere, hydrosphere, and cryosphere.” Peixoto and Oort make an important distinction between *climate* and *weather*: “The climate . . . can be considered as ‘average weather,’ complete with some measures of the variability of its elements and with information on extreme events.” Some variables that are relevant in weather and in other branches of meteorology are also important in the characterization of climate.

In spite of its title, agency budget pressures and prioritization decisions have greatly reduced the scope of the USGCRP from *global change* to the narrower *global climate change*. Some areas of scientific research and remote sensing that have near-term scientific importance or serve practical applications have been reduced in scope or eliminated (e.g., land surface and vegetation research employing high-spatial-, high-spectral-resolution electrooptic sensors and microwave and laser sensing of oceans and ice).

When the current program was formulated, it was assumed that Landsat-like data would continue to be available. Now, however, it is the view of the CES that the combination of the loss of Landsat-6 and the cancellation of other advanced electrooptical sensors leaves a substantial gap in medium- to high-spectral-resolution and high-spatial-resolution measurements needed to meet the USGCRP objectives. Global surveys of vegetation class, land use, and surface minerals, as well as contributions to cartography, are not included in current plans. As structured, the USGCRP will also not generate some other data of substantial value to the earth applications community, such as systematic regional and global topography, digital elevation maps, and magnetic and gravitational fields.

The active microwave oceanographic and cryospheric measurements for the USGCRP (altimetry, scatterometry, and synthetic aperture radar, or SAR) are not integrated into a coherent plan either for their eventual transfer to long-term research or operational status on future NOAA satellites or for securing the timely availability of these data from foreign and/or internationally cooperative systems.

Alterations in vegetation class and extent are obviously changes in surface properties of great human interest; they are also changes that can be monitored from space. Such changes may develop because of climate change. The reduction in scope of the USGCRP to focus more narrowly on global climate change reduces the capability to make these measurements. At the same time that reductions in scope have been directed, policymakers are asking for nearer-term answers upon which to base decisions—in effect, re-expanding the scope of the effort.

The previously mentioned reductions now make the United States increasingly reliant on other nations for spacecraft, sensor systems, and data in some areas in which the United States was preeminent, notably high-spectral- and high-spatial-resolution electrooptical and microwave measurements. The CES enthusiastically endorses the concept of sharing the burden for the conduct of earth observations among as many nations as possible. However, the diminution of the ability of the United States to obtain required data from its own systems places a greater importance on the reliability of international agreements than in the past. In the past, these agreements have been difficult to reach and have not always resulted in ready data availability. This difficulty has been present in even such commonplace data as coarse-spatial-resolution meteorological measurements.

The USGCRP includes efforts of NASA, NOAA, the Department of Interior (DOI), Department of Agriculture (USDA), Department of Energy (DOE), Environmental Protection Agency (EPA), National Science Foundation (NSF), and others. As a result, responsibility for the USGCRP involves more than a dozen agencies, their respective budget examiners, and several dozen congressional committees, making efficient management improbable, or at least quite difficult.

USGCRP oversight is carried out by an interagency committee, which has provided useful coordination, but which also has little authority, offers inadequate means to review progress against milestones, and must rely on its powers of persuasion to conduct normal program management functions. For example, reallocation or redirection of program elements in an efficient manner (in response to changing needs or new discoveries) is hampered by the number of agencies and budget processes involved.

APPLICATIONS PROGRAMS

Advancing the operational utility of civil earth observations has not been given national priority in the current efforts. In the environmental satellite programs of NOAA, no systematic program has been formed to replace the NASA Operational Satellite Improvement Program (OSIP), which was abruptly terminated early in the 1980s and upon which NOAA had relied. In its stead, the U.S. operational weather satellite program has only a limited internal

research and development arm addressing needed improvements in the current generation of sensor systems and requirements for the next generation of sensors. New sensors are being pursued under the EOS program, but—despite some NOAA participation—with relatively little consideration for their affordability or practicality for transfer to operational use.

Under current NOAA planning, only small or incremental improvement of operational capabilities will take place over the next decade or even longer, even though the nations of the world plan to launch some 50 new observation satellites in that period (BNSC, 1992). Indeed, the polar-orbiting satellites of NOAA are little changed since the launch of the Advanced Tiros-N (ATN) in 1978. Thus, a period of some 27 years has elapsed between generations during a time of great technological advancement.

Of particular significance, no plans yet exist for the incorporation in the operational U.S. weather satellite fleet of advanced microwave instrumentation for altimetry, measurement of sea surface winds and waves, and imaging of polar ice fields—or, if not carried on U.S. satellites, for ensuring the routine availability of these data to operational users from foreign satellite systems. The CES questions the absence of an organized, advanced R&D program for the systematic introduction of these appropriate new technologies.

Landsat-like data are a necessary element in global change research and in operational applications. In spite of that, the future availability of these data is not ensured as of early 1995. The current U.S. role in this area rests solely with the aging Landsats-4 and -5. The future availability of these data to U.S. users rests on a Landsat-7 satellite planned to be launched in 1998 and on the commercial and data exchange practices of foreign countries. Similarly, as noted in the preceding section on the USGCRP with regard to research, no plans exist for the exploitation of space-based imagery in the preparation of topographic maps, digital elevation models, global vegetation inventories, mineral and soil surveys, or updated standard maps. Such products are among the greatest benefits to be derived from the nation's investment in its space program.

The problems associated with space applications in general, and with earth observations in particular, are of long standing. Ten years ago, the NRC's Space Applications Board (SAB) wrote (SAB, 1985),

The civil space program of the United States is a study in contrasts. The shuttle program is now operational; funding for the space station has been included in the President's FY 1986 budget. In the field of science, the NASA program in physics and astronomy (for example) is strong and has received increases in funding. Several NASA research programs involving earth observations, the Upper Atmosphere Research Satellite (UARS), the Earth Radiation Budget Experiment (ERBE), and the development of an instrument to measure wind speed at the ocean surface are moving ahead vigorously.

There is, however, one major sector of the space program that is in disarray: the operational remote sensing of the earth. The successful weather satellite system in NOAA has been severely affected by programmatic reductions, by stretch-outs in satellite procurement, and by reduced cooperation between NOAA and NASA. The land remote sensing effort is endangered as the attempt to turn the program over to the private sector threatens to flounder because of limitations placed on federal support. No civil operational program in ocean remote sensing is in place or planned, although the Navy (with the cooperation of NASA and to a lesser degree NOAA) plans to mount a significant effort, the Navy Remote Ocean Sensing Satellite (NROSS).

Information from operational earth remote sensing systems is needed for a host of practical purposes, such as weather forecasting, ocean transportation and utilization, land management, and mineral exploration. This information is also required to improve understanding of various earth sciences—meteorology, oceanography, geology, and geophysics. Not only practical applications of substantial economic importance but also the advance of earth-oriented science are inhibited by the inadequacies of this sector of the space program.

Why should such a practical program be floundering? Why is it that earth-oriented activities are being outdistanced by other, less applicable sectors of the space program? It is true that the surge into space is largely an investment in the future, but one might assume that we as a nation would make every effort to reap the benefits of our investment as soon as it became possible to do so. This is not being done. Indeed, the situation is even less logical than has already been stated: In at least one critical area of earth remote sensing, the United States is marking time as other countries move toward world leadership and prepare to reap the benefits of our investment—using technology developed in this country.

... We do not condone or accept as appropriate the disarray in operational earth remote sensing.

These observations remain valid a decade later.

SMALLER SATELLITES AND EARTH PROBES

The CES believes that it is desirable for the nation's civil earth observations program to include both long-term elements and other elements that permit quick-response, rapid-turnaround measurements. Satellites of all sizes are likely to be required, with each filling a particular niche.

Smaller satellites can play a scientifically important role in the MTPE and can also play important operational roles. The CES and other bodies within the NRC have long endorsed the use of smaller missions (see, e.g., SSB, 1988a,b, 1991; ASEB, 1994). The CES notes, however, that the choice of satellite size should be made by applying well-understood systems engineering procedures to the task to be accomplished. The very large polar platforms proposed for the MTPE in the early 1980s were a mistake. In retreating from that mistake, however, the pendulum must not be allowed to swing so far in the opposite direction that we lose sight of the function the satellite is intended to serve. The program should not be detrimentally driven by the pursuit of still another technological objective.

Furthermore, the CES sees no pressing need within the domain of civil earth observations to fly satellites as a management demonstration simply to prove that small, modestly capable satellites can be launched quickly. Well-crafted experimental technology satellites, however, can serve as engineering testbeds to qualify new subsystems for later use in research and operational observation systems; but such testbeds are quite likely to be of a different character than satellites intended to observe long-term changes in climate variables. NASA's Advanced Communications Technology Satellite (ACTS) is illustrative of the difference: it demonstrates new technologies for later adoption, but is not itself in a form that would permit its use or replication as an operational commercial communications satellite. CES concerns arise only when mission objectives are mixed and demonstrations of new technology are appended as a requirement for the conduct of a mission in such a manner as to thwart scientific objectives or add unwarranted costs and delays. Astronaut-serviceable platforms should not be mandated as a requirement, and the use of microsatellites should not be mandated either.

It is important to recognize that technology changes and that the boundary between what measurement requires a larger satellite and what can be accomplished with a smaller satellite is not static. Some technological areas will be advanced with ease, making the boundary quite dynamic, while other areas will prove more resistant to advances—or will encounter basic limits likely to be of long standing. Sound and objective engineering judgment must be applied to determine the location of the boundary in individual cases.

EARTH SYSTEM MODELING AND INFORMATION SYSTEMS

Large information systems and accurate earth system models are easier to imagine than they are to build. Constructing earth system models possessing predictive skill of a useful degree is among the most challenging research tasks humans have undertaken. As with information systems, the state of earth system modeling is rudimentary, and the production of models must proceed incrementally, allowing for frequent adjustments in approach. The mathematical methods, algorithms, visualization approaches, software design, and other aspects of earth system modeling all have major embedded research tasks. Furthermore, present and probable future modeling capabilities should be factored into the design of observational systems. Both models and observational systems should be designed such that—as far as is reasonably possible—continuity, sampling frequency, and accuracies are commensurate with the needs for understanding and simulating the behaviors of the components of the earth system.

The subject of earth system modeling has received considerable attention by NRC bodies. The CES has itself addressed this issue frequently (SSB, 1982, 1985). The U.S. Committee for the IGBP (the Committee on Global Change, or CGC) has written extensively on the complexities of the problem of modeling (CGC, 1986, 1988, 1990). The Bretherton report contains comprehensive diagrammatic representations of the earth system model (NASA, 1988). The NRC's Board on Atmospheric Sciences and Climate (BASC) has addressed modeling in a number of its reports, but perhaps the most significant is its 1991 data assimilation report (BASC, 1991), which addressed the integration of asynchronous, irregularly distributed measurements of varying quality and character. NASA also convened a special climate modeling workshop whose final report provided a broad survey of the challenges and requirements alluded to above (Unninayar and Bergman, 1993). All of these reports have stressed the intellectual challenge of the tasks before the MTPE and USGCRP.

Perhaps fittingly, in light of the difficulty of the research it supports, the EOS Data and Information System (EOSDIS) is the most complex civil data and information system yet conceived (Dutton, 1989). The task before NASA is not one of preparing a system specification, hiring the software and hardware systems specialists, and then awaiting the completion of a "turnkey" system, although there are elements of each of these. Instead, the task involves major, continuing research efforts as well. While some overall structures can be beneficially proposed and followed, the CES believes that development must proceed incrementally and must allow for frequent adjustments in direction as lessons are learned.

Data systems have been a perennial problem for the space and earth sciences for the entire history of the space program (see, e.g., Zygielbaum, 1993). The CES's choice to limit this historical review to the period beginning in the early 1980s neglects much of that history. The lessons learned have often been painful ones, with the early Landsat experience being particularly noteworthy. As a result, the NRC has written numerous reports and critiques of information systems (see, e.g., SSB, 1982, 1985, 1986, 1988a,b; CODMAC, 1982, 1986, 1988; NRC, 1990; and many others). Most recently, the NRC was asked to form a panel specifically to review the plans for EOSDIS. It was chaired by Charles Zraket, and its report called for a major revision of EOSDIS (NRC, 1994). The CES concurs with the Zraket panel's recommendations. In brief, the recommendations call for a user-driven design that is logically distributed and based on an open and fully extensible architecture. Those recommendations have been accepted by NASA.

ASSOCIATED ACTIVITIES

There are a number of topics that do not fit neatly into the above categories. They include magnetic and gravitational field measurements, the use of the Global Positioning System (GPS), the importance of coordinated in situ measurements, and the role of the research and analysis (R&A) line in the NASA budget.

Over the long term, an enhanced measurement of the Earth's magnetic and gravitational fields will be a vital ingredient in global change research and operational applications. The history of the measurement of these fields by satellite is populated by more failed proposals than successful missions. The current USGCRP largely neglects the solid earth and its associated fields.

The advances being made in the application of the GPS may offer a lower-cost way to achieve important earth science and applications objectives. These include applications in many areas of the earth sciences. The NRC has considered some of these applications in analyzing GPS enhancements (ASEB, 1995).

No space-based system is entirely self-sufficient; all rely on in situ measurements, and all rely on advanced R&A for the interpretation of the results of space-derived data. Support for in situ measurements and experimental campaigns is widely dispersed in the government. The CES believes that it is vital that in situ measurements and campaigns be closely coordinated with the deployment of space-based systems, and that each is less effective if technical or budget problems thwart their parallel deployment where necessary. For example, intended support from the NASA Scatterometer (NSCAT) to investigations of the El Niño was precluded when NSCAT's launch was delayed until after the period of deployment of the in situ sensors.

The NASA budget contains a category termed *R&A*. *R&A* has customarily supplied funds for enhancing fundamental understanding in a discipline and stimulating the questions from which new scientific investigations flow. *R&A* studies also enable conversion of raw instrument data into fields of geophysical variables and are an essential component in support of the research required to convert data analyses to trends, processes, and improvements in simulation models. They are likewise necessary for improving calibrations and evaluating the limits of both remote and in situ data. Without adequate *R&A*, the large and complex task of acquiring, processing, and archiving geophysical data would go for naught. Finally, the next generation of earth scientists, the graduate students in universities, are often educated by performing research that has originated in *R&A* efforts.

THE ORGANIZATION OF CIVIL EARTH OBSERVATIONS PROGRAMS

As noted above, the United States is funding a variety of experimental and operational earth observations programs. They include EOS, Earth Probes, NOAA's polar-orbiting and geostationary operational satellites, Landsat, and the DMSP. Of these, only the NOAA and DMSP polar orbiters are currently planned to be merged, although Vice President Gore's National Performance Review recommended the further merger with the NASA

MTPE satellites as well (Gore, 1993). At present, systems engineering practices are not being applied to the total U.S. satellite constellation. Overlapping functions suggest that economies of scale may be found in mergers of several or all of these systems. In addition, the oversight of both the earth sciences and operational applications support provided by these systems is fragmented. A full spectrum of options exists for reducing this fragmentation, ranging from maintaining the status quo (no improvement) to forming a single entity to oversee all civil earth observations.

International collaborations are an important part of both NASA and NOAA programs. In the area of international telecommunications, similar interdependencies led to the formation first of Intelsat and then of Inmarsat. Were the United States to take the step of creating a designated entity for civil earth observations, it might prompt other nations to do the same and possibly lead to a sharing of responsibilities analogous to that in the telecommunications industry. While there seems little prospect of near-term movement in this direction, it nevertheless seems unlikely that nations will permanently accept the high costs of orbiting national systems, when the same data could be gained from shared systems at lower cost.

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3.5 A Scientific Assessment of a New Technology Orbital Telescope

A Report of the Task Group on BMDO New Technology Orbital Observatory¹

EXECUTIVE SUMMARY

The end of the Cold War and a decline in the fortunes for space research have gone hand in hand. If the space sciences are to continue advancing and not slip into a decline matching their dwindling budgets, new and innovative ways will have to be found to perform space missions. This is particularly true for space astronomy, given that certain features of spacecraft design (e.g., telescope apertures and detector sizes) are constrained by the laws of physics and cannot be miniaturized and still carry out their scientific tasks.

Not all of the events of recent years have been detrimental for space science. The decline in superpower rivalries has opened new avenues for international cooperation. Similarly, once-secret military technology has become available for civilian applications. Indeed, declining defense and space budgets have given rise to hybrid projects with both military and scientific goals. Prime among these was the recent Clementine lunar orbiter. This report assesses another such project, a large space telescope. In addition to demonstrating technology of interest to the Department of Defense (DOD), this mission has significant scientific capabilities, both in enabling direct astronomical observations and in demonstrating technology that may drastically alter the cost/performance ratio of future NASA missions.

At the height of the Cold War, the DOD's Strategic Defense Initiative Organization (SDIO) actively sponsored development of the technology needed to make space-based laser weapons feasible. As part of this program, SDIO developed many components of an agile, ultra-lightweight, 4-meter space telescope, equipped with an advanced active-optics system. Budgetary shortfalls and program redirection led to the cancellation of any tests of laser weapons in space. However, many components of the system had other applications of interest to both the DOD and the scientific community, and so development of the space-based telescope continued under the so-called Advanced Technology Demonstrator (ATD) program of SDIO's successor, the Ballistic Missile Defense Organization (BMDO).

To assist in the evaluation of the ATD's scientific potential, BMDO asked the Space Studies Board to provide advice on instrumentation, data management, and science operations to optimize the scientific value of a 4-meter mission. Following the initiation of the study by the Task Group on BMDO New Technology Orbital Observatory, however, a combination of budgetary pressures and redirected defense priorities forced BMDO to defer the ATD mission. Nevertheless, BMDO reaffirmed that "planning advice and recommendations [about the scientific aspects of the 4-meter mission] would still be valuable in formulating future joint experiments should this program or a similar one be funded in a subsequent Defense Plan."²

Despite the uncertain future of a flight-test of the 4-meter telescope and the currently unknown national security goals of such a mission, the task group proceeded to analyze the astronomical potential of the deferred mission. Given the potential scientific aspects of the 4-meter telescope, this project is referred to as the New Technology Orbital Telescope (NTOT), or as the ATD/NTOT, to emphasize its dual-use character. The task group emphasizes that it was specifically charged to assess the astronomical capability of the ATD/NTOT and therefore included only people with competence for that specific assessment.

The ATD/NTOT mission was conceived as a low-cost demonstration of technology, intended for use in future national security spacecraft, but having implications for astronomy. As such it is:

- Designed to cost (~\$350 million, including launch);
- Uses existing technology and/or designs wherever possible;
- Has a 3-year development schedule and a nominal orbital lifetime of 1 year; and
- Is not driven by specific astronomical requirements.

¹"Executive Summary" reprinted from *A Scientific Assessment of a New Technology Orbital Telescope*, National Academy Press, Washington, D.C., 1995, pp. 1-6.

²Payton, Gary E., Deputy for Technology Readiness, BMDO, memorandum to the Space Studies Board, October 24, 1994.

Given these characteristics, the task group adopted the basic philosophy that any potential involvement of the astronomical community in the ATD/NTOT should, at least initially, be predicated on the assumption that the ATD/NTOT is primarily a test of new technology for astronomy and is not a mission driven by any particular astronomical requirements. In this light, the ATD/NTOT's greatest benefit to the astronomical community will be to show whether or not it is possible to break the Hubble paradigm—that is, to demonstrate that it is possible to obtain large space optics at low cost. While doing this, it could carry out major astronomical studies not possible with the HST even with its currently planned improvements in instrumentation.

The basic features of the ATD/NTOT are the following:

- A 4-meter-aperture, 17-mm-thick, primary mirror equipped with some 260 actuators for on-orbit refiguring;
- Afocal optics with an image-stabilization mirror located at an image of the entrance pupil to adjust the pointing anywhere within a ± 5.7 -arc-minute region without moving the spacecraft;
- Graphite polycyanate (graphite epoxy) structures for the entire telescope assembly;
- Use of an on-board inertial reference to maintain pointing stability over a bandwidth from 1 to 300 Hz;
- The ability to track stars, as faint as 19th magnitude, through the full aperture of the telescope to maintain pointing stability against disturbances at frequencies of less than about 10 Hz;
- A design optimized for agility and rapid slewing from one part of the sky to another; and
- A highly eccentric orbit with a 12-hour period allowing continuous viewing of targets over much of the sky for periods up to about 8 hours.

Estimates of the performance of the ATD/NTOT suggest that it approaches the diffraction limit at near-infrared wavelengths. In the optical, its full width at half maximum (FWHM) is better than that for any current or planned facility, while the diameter for 50% encircled energy is comparable to the HST's. The reason for this is the relative roughness of the primary mirror. In fact, the primary mirror dominates all other sources of wavefront errors in the ATD/NTOT's error budget. This suggests an obvious enhancement: improving the figure of the primary mirror by a factor of two so that its contribution to the telescope-level error budget is comparable to that of the other components. Not only could this improvement be achieved at relatively low cost, but it would also have a dramatic impact on the ability of the ATD/NTOT to do both the technology demonstrations and the observing projects outlined in this report.

The baseline instrument package for the ATD/NTOT consists of a variety of optical- and infrared-array detectors. The one of most interest is a 1024×1024 indium antimonide (InSb) infrared array that would have state-of-the-art astronomical capabilities if operated at a cold enough temperature. The two passive, visible, fine-tracking arrays would have some astronomical applications. These would, however, be limited because their charge-coupled devices (CCDs) are line-transfer devices, and the arrays and their amplifiers are not optimized for low readout noise. The obvious deficiency in the instrument package is the absence of an optical framing camera of astronomical quality. The addition of such an instrument would have a very significant impact on the astronomical capabilities of the ATD/NTOT.

Understanding the areas in which the ATD/NTOT might have significant advantages over existing and planned facilities is critical to deciding which scientific projects and technological demonstrations to emphasize. To do this the task group considered two aspects of the ATD/NTOT: its optical and near-infrared performance, and the operational modes in which it can be used most cost effectively. Consideration of performance factors led to the conclusion that the ATD/NTOT has major advantages:

- In the near infrared (2 to 4 microns), where the sky background is reduced by several orders of magnitude;
- In the far red (>0.7 micron), where the sky background is reduced by one order of magnitude;
- For programs that depend on high contrast between a point source and its neighborhood, or those that require subarc-second spatial resolution; and
- For programs that are photon-starved, that is, receive little attention with the HST.

Consideration of operational factors led to the conclusion that the ATD/NTOT is best suited to large surveys because repeated use of the telescope in a single mode, by a small team of scientists, is the most cost-effective operating procedure. In addition, large surveys make less than optimum use of complex, multiuser facilities such as the HST.

In determining what scientific and technological projects the ATD/NTOT is most suited to perform, the task group's overriding priority has been to minimize cost while still ensuring the capability to do exciting science. The first astronomical goal of an ATD/NTOT flight should be to test the applicability of its technology for use in future space science missions. Prime among these tests are (in no particular order):

- Demonstration by actual astronomical application of the ability to adequately refigure a large mirror in orbit to obtain astronomical-quality images, both with and without ground-based intervention;
- Evaluation of image quality and its consistency both across the field of view as the fast-steering mirror stabilizes the telescope's line of sight and in a variety of thermal environments;
- Characterization of the likely degree of passive cooling by establishing the thermal emission from the optical and other components of the system in both the initial low Earth orbit and the eventual highly elliptical (or Molniya) orbit;
- Investigation of the stability of field distortions, particularly as the figure of the primary mirror responds to its control actuators and, also, as the fast-steering mirror stabilizes the telescope's line of sight;
- Exploitation of the ATD/NTOT's agility and large fuel reserves to actively maneuver the spacecraft so that it is in the right place, at the right time, to observe ephemeral events such as occultations;
- Exploration of the possibilities presented by the Molniya orbit to conduct very long integrations in a cost-effective manner; and
- Utilization of the facility as an experimental testbed for various modes of ground operations that may be needed for future space science missions.

The implementation of some of the task group's suggested enhancements to the baseline ATD/NTOT would significantly improve the evaluation of both the technology and the astronomical significance of a program of technology demonstrations. Two of the most important enhancements are enhancing the figure of the primary mirror and adding an optical framing camera. These improvements would allow far more rigorous tests of, for example, the image quality that can be realized with the ATD/NTOT technology. Addition of just the optical framing camera would permit studies of the system's photometric stability. Also of great importance is enhancing the cooling of the InSb array, since this would allow a better evaluation of the ATD/NTOT's infrared performance.

If the ATD/NTOT's technology passes its key tests and can exceed its 1-year design lifetime, then it will have a significant capability for astronomical research. An area in which the ATD/NTOT should excel is in studies of origins. The creation and evolution of the universe and its component galaxies, stars, and planets is a topic of great scientific and popular interest and one in which large-scale surveys play major roles. To highlight the ATD/NTOT's potential in these areas, the task group discusses four possible observing programs:

- A series of deep surveys of the early universe at near infrared wavelengths to study the evolution of galaxies, define the magnitude/number-count relation, and search for new "standard candles" at intermediate redshifts. All of these projects are consistent with the baseline mission but would benefit significantly from the addition of an optical framing camera and enhancement of the primary mirror.
- A survey of the outer solar system to define the size- and radial-distributions of the primitive bodies constituting the Kuiper Disk (down to ~1 km bodies at ~40 AU). This project requires an optical framing camera and would benefit significantly from the use of an enhanced primary mirror.
- High-resolution optical studies of the disks, jets, and winds associated with young stellar objects during the embedded, accretion-dominated, and post-accretion phases of their evolution. The success of this project depends critically on enhancement of the primary mirror and the addition of an optical framing camera.
- Synoptic occultation observations of Pluto and Triton to monitor global atmospheric variations due to seasonal variations in insolation. Although compatible with the baseline mission, this project may be expensive in terms of operations and use of spacecraft resources because it would involve extensive spacecraft maneuvering and orbital changes.

In the course of the task group's deliberations, a number of items arose that raise questions about the astronomical utility of the ATD/NTOT. In particular:

- Little or no systems analysis has been performed to verify that the ATD/NTOT's individual components can be combined to form a working astronomical telescope.
- There is some doubt about the ability of the baseline ATD/NTOT to track guide stars as faint as would be needed for certain observations. Although there are solutions to this problem, the problem may be moot if the telescope is devoted to surveys.
- Aspects of the ATD/NTOT's design, particularly that of the tripod supporting the secondary mirror, may scatter stray light into the focal plane.
- The rate at which the figure of the primary mirror will need correction as it deforms due to thermal and other drivers, and the impact corrections may have on observing overhead, are not clear.
- Software problems of the type that ultimately doomed the Clementine mission must be avoided.
- The effect of cosmic-ray events on the ATD/NTOT's imaging arrays when the spacecraft is operating in the highly eccentric Molniya orbit is a significant factor.
- Although building to cost is becoming a key feature of NASA's present and future missions, neither NASA nor the space science community has much experience with this mode of operation.

While the resolution of these issues is beyond the scope of this study, they must nevertheless be resolved as the ATD/NTOT mission is further defined.

The task group's analysis shows that the ATD/NTOT mission uses advanced technology that has important potential applications for future space astronomy missions. Furthermore, its advertised cost-effectiveness is crucial to NASA's ability to carry out significant space astronomy missions in an era of tightly constrained budgets. Both of these factors have not escaped the notice of other groups. Thus, both the High-Z and Polar Stratospheric Telescope concepts draw heavily on the capabilities of the ATD/NTOT's technology. The task group's basic conclusion is that the ATD/NTOT mission does have the potential for contributing in a major way to astronomical goals. It is equally clear that if the ATD/NTOT performs as advertised, it could undertake astronomical observations that could not be matched by any other facility now in existence or under development. Thus the task group's first and foremost recommendation is as follows:

1. To optimize the return to astronomy from the ATD/NTOT, the astronomical community should be directly involved in the continued study and development of this mission, including system engineering and complete mission analysis. These community representatives should be selected by NASA, and their role should be to advise NASA on the continuing value of this mission for astronomy. The group should include not only astronomers proposing specific observing programs, but also individuals with particular expertise in the design of large telescopes and space missions.

Since any scientific applications of the ATD/NTOT are a bonus, the task group further recommends the following:

2. If the ATD/NTOT mission flies, a suite of tests of the suitability of its technology for astronomical applications should be carried out. Some of these tests can be conducted concurrently with DOD's demonstration mission, but others require an astronomical phase of the mission.
3. Although scientific goals must be kept in mind and accommodated insofar as possible during the planning of the ATD/NTOT mission, these goals should not impose requirements that would have a major impact on development or operations costs.
4. The ATD/NTOT's astronomical promise is sufficient, even at this preliminary stage, that it is appropriate to plan for a mission phase devoted to astronomical observations. The resources devoted to planning an astronomical mission should be kept to a minimum until such time as the ATD/NTOT's scientific and technological capabilities are better defined.

A mission phase dedicated to astronomical observations, while highly desirable, could be extremely expensive if not managed appropriately. Given the philosophy of designing to cost, the development costs for astronomical

research programs must be kept to an absolute minimum. In order to minimize operational costs, the task group recommends the following:

5. The astronomical phase of the ATD/NTOT mission should be carried out by a principal investigator and a science team, with rotating membership to accommodate a range of scientific expertise. No provision should be made for a traditional guest observer program.

6. An extended ATD/NTOT mission should concentrate on extensive surveys that repeatedly use the ATD/NTOT in a single mode.

7. Astronomical data collected in the ATD/NTOT mission should be delivered promptly to an existing public archive that is independent of and expected to outlive the mission.

Because DOD sponsorship of the ATD/NTOT is uncertain, the mission's exact specifications are unclear. The task group has assumed a baseline performance predicated on the requirements necessary to perform the mission that BMDO has now deferred. This analysis revealed several areas in which enhancements beyond the baseline specifications would have a significant impact on the ATD/NTOT's astronomical capabilities. Some of these enhancements may ultimately be required by, or at least be consistent with, a DOD mission if and when it is finally defined. Although the task group has not evaluated the cost-effectiveness of all of these enhancements (something that must be done during the system engineering phases of the mission), it has discussed their potential impact with representatives of Lockheed and Itek. In one case the costs are well defined and the performance benefits reasonably determined. In other cases, neither the costs nor the actual improvements in performance are very well determined. It is, however, the sense of this task group that these enhancements are likely to be very cost-effective and important for the astronomical aspects of the mission. In particular:

8. The figure of the ATD/NTOT's primary mirror should be improved by roughly a factor of two to reduce its surface error to ~ 17 nm, and, thus, the total system's wavefront error to roughly 50 nm rms. Itek estimates that this enhancement would cost $\sim \$100,000$.

9. A large-format, framing, optical CCD of astronomical quality should be included in the ATD/NTOT's focal-plane package. A less expensive but clearly less desirable option would be to replace the baseline line-transfer CCDs in the fine-tracking sensors with frame-transfer CCDs.

10. The ATD/NTOT's baseline infrared detector, an InSb array, should be optimized for sensitivity by, for example, additional cooling and by minimizing the number of emitting surfaces in the optical path.

The task group briefly discussed other enhancements that could significantly improve the scientific return from an extended mission dedicated to astronomical observations. Unlike those discussed above, however, all of these modifications would add significantly to the cost of the mission and/or perhaps be incompatible with the national security objectives of the mission. These enhancements include:

- Modifying the ATD/NTOT's orbit to minimize and/or stabilize the thermal load on the spacecraft;
- Optimizing the design to enhance the passive-cooling characteristics of the telescope and focal-plane instruments; and
- Adding additional instruments (e.g., a dedicated infrared focal plane) for scientific research.

Even if the ATD/NTOT mission is not eventually funded by BMDO and does not find another sponsor in the national security community, the task group believes that the time devoted to this study has been of use. While the complete package of technologies embodied in the ATD/NTOT proposal promises exciting advances in astronomical capabilities, it should be remembered also that much of its hardware already exists. A complete adaptive-optics system and examples of thin primary mirrors, for instance, currently sit gathering dust in testing chambers. Many of these subsystems are themselves interesting additions to the tools at the disposal of astronomers and may find scientific applications very different from those for which they were designed. The ATD/NTOT may never fly, but if this report does nothing more than illuminate some of the capabilities lurking in the shadows of the Cold War, it will have achieved something worthwhile.

3.6 Managing the Space Sciences

A Report of the Committee on the Future of Space Science¹

EXECUTIVE SUMMARY

In April 1994 the National Research Council received a request from NASA Administrator Daniel S. Goldin that the NRC's Space Studies Board provide guidance on several questions relating to the management of NASA's programs in the space sciences.² The issues raised in the Administrator's request closely reflect questions posed in the agency's fiscal year 1994 Senate appropriations report. These questions included the following:

- Should all the NASA space science programs be gathered into a "National Institute for Space Science"?
- What other organizational changes might be made to improve the coordination and oversight of NASA space science programs?
- What processes should be used for establishing interdisciplinary science priorities based on scientific merit and other criteria, while ensuring opportunities for newer fields and disciplines to emerge?
- What steps could be taken to improve utilization of advanced technologies in future science missions?

Since the creation of NASA in 1958, space science has been a key element of its mission. Indeed, the Augustine Committee report,³ submitted at the end of 1990, asserted that science was NASA's most important mission. The committee responsible for the present report has proceeded on the same premise. A balanced and healthy program of space science is crucial to the future of NASA, regardless of the overall level of support available to the agency.

The most important recommendations of this report are listed below. They are further elaborated following the list.

- NASA should not establish a "National Institute for Space Science" that would pull together the three present science program offices.
- NASA should augment the responsibilities and authorities of the NASA Chief Scientist.
- NASA should establish a set of fair, open, and understandable processes to be used in the prioritization of space science research. These processes will ensure that major project proposals considered at progressively higher levels within the agency have the heritage of scientific merit that comes from a successful confrontation with competing proposals at lower levels.
- NASA should create a comprehensive strategy and plan for the technologies that support the space sciences, with the responsibility for near-term technology development residing in the science programs to be served and the responsibility for longer-term technology strategy and development residing in the Office of Space Access and Technology.
- NASA should change the funding of its field centers to full-cost accounting ("industrial funding"). Cost accounting should be based on full program costs, including civil service salaries. The committee endorses NASA's intentions to move in this direction.
- NASA should exercise caution in downsizing its Headquarters staff and transferring functions to the centers; this process could be carried too far and have unintended consequences. The committee identified a number of areas where it believes control should be retained at Headquarters.
- NASA science budgets should include a limited amount of dedicated funding for innovative ideas in high-risk, high-return areas lying outside the current framework of inquiry or design.
- NASA should take a cautious approach to the recently proposed establishment of focused science institutes. There should be a well-defined process for their selection and creation, and a clear plan for the phased transfer of base funds to programmatic funding.

¹"Executive Summary" reprinted from *Managing the Space Sciences*, National Academy Press, Washington, D.C., 1995, pp. 1-4.

²In this report, "space sciences" refers to all of NASA's science programs conducted in or from space, including space astronomy, space physics, planetary exploration, microgravity research, space life sciences, and Earth science.

³Advisory Committee on the Future of the U.S. Space Program, *Report of the Advisory Committee on the Future of the U.S. Space Program*, December 1990.

The following expands key recommendations of the report:

Institute for Space Science—In response to direction in the FY 1994 Senate appropriations report, the committee considered a space sciences umbrella organization within NASA to coordinate and oversee all space science activities, functioning like the National Institutes of Health (NIH) within the Department of Health and Human Services. The committee reviewed the advantages and disadvantages of such a model and concluded that the NIH model, while effective in the arena of health research, is not appropriate for the space sciences. NASA space science benefits from close coordination with other elements of NASA, such as hardware development, launch services, and tracking and data operations, which have no counterparts in the NIH model. The committee believes the required coordination would be hampered by the creation of a quasi-autonomous space science institute. The committee therefore does not recommend establishment of such an umbrella institute.

The Role of the Chief Scientist—The role of the Chief Scientist was found to be a critical one from many perspectives, leading the committee to recommend expanding the authorities and responsibilities of this position. Despite the central role of the science associate administrators in the management of their respective science areas, the committee finds a need for greater integration and coordination of these programs. To achieve this, the position of Chief Scientist should be strengthened, particularly by the addition of concurrence authority in key matters affecting space science. The Chief Scientist should be a person of eminent standing in the scientific community with a significant record of accomplishment. A proposed “functional statement” for the Chief Scientist is given in Chapter 4. A major component of this official’s integration responsibility is coordination and oversight of the recommended science prioritization process. Another component is coordination of the technology development programs that support space science.

The Prioritization Process—The committee believes that peer review is the most effective form of merit review for the selection of scientific research. A clear set of criteria, known and understood by all parties, is crucial to the prioritization of scientific goals. The relative ranking of science and mission plans will be most strongly affected by scientific factors at the entry level, where proposals from the same discipline or subdiscipline compete against one another. As the arena of competition broadens to the interdisciplinary and then to the agency-wide level, other programmatic and political influences become increasingly important. It is essential, however, that all proposals being considered at progressively higher levels retain the heritage of scientific merit that comes from successful confrontation with their peers at lower levels. The office of the Chief Scientist should oversee these prioritization processes, especially as they cross disciplinary boundaries. NASA management should cancel those programs or projects that are failing or whose priority has dropped substantially in this prioritization process. The committee found that peer review and the above corollary principles apply generally to technology research as well.

Technology Planning—New technologies are important as agents of change, enhancing the quality of scientific output and the ability to accomplish more with less. Technology development is undertaken both by NASA’s science program offices and by its Office of Space Access and Technology (OSAT). The committee recommends that NASA establish an agency-wide strategy and plan for the technologies that support the space sciences. These technologies may be characterized as near-term or far-term technologies (the latter defined as requiring more than five years to be ready for flight demonstration). The space science offices should have primary responsibility for identifying and reviewing near-term technologies, giving them greatest control of the technologies that most immediately affect the success of their programs. Each science office should allocate a significant fraction of its resources to Advanced Technology Development activities and should be willing to pool resources to achieve shared objectives. Most importantly, the implementation of all categories of technology development should be undertaken by the best-qualified individuals or teams within NASA, other government laboratories, industry, or academia, as determined by peer review.

Promising far-term technologies should be identified, funded, and managed by OSAT. Projects in these areas should be reviewed jointly by the science offices and by OSAT. Like near-term technology development, far-term projects should be carried out by the best-qualified individual or teams, as determined by peer review. These projects should stimulate exploratory development of possibly unconventional technologies having the potential of producing breakthroughs in capability. Finally, a rigorous review process should be put in place to identify those projects that ought to be terminated in the present constrained budgetary environment.

“Industrial Funding”—The committee examined the advantages and disadvantages of an explicit full-cost accounting system in which all charges, including salaries and facilities, are charged against projects (so-called “industrial funding”). This approach permits ready assessment of comparative costs that might otherwise be hidden

in an institutional funding environment. The committee endorses NASA's decision (stated in the "Zero Base Review" briefing to the Congress) to identify, budget, and manage by total program costs, including civil service labor costs. The committee recommends that NASA change the funding of its field centers to an industrial funding arrangement. The committee believes that decisions on program priorities and budgets would be more rational if based on full-cost accounting, and program accountability and discipline in personnel management would thereby be enhanced. A similar recommendation was made in the *NASA Federal Laboratory Review* report.⁴

The Downsizing of Headquarters—NASA is currently "re-engineering" its organization. This re-engineering entails a very large downsizing of its Headquarters staff and a concurrent transfer of functions to the centers. The result is expected to be the analog of a lean "corporate management" model. While the committee endorses the intent, it notes that an unintended consequence could be a center-dominated model as opposed to the desired enterprise-focused one. Several recommendations are offered to avert this outcome. Not all program management functions should be transferred to centers. Those complex programs that cut across centers should be retained at Headquarters and integrated with enterprise management. Support of scientific disciplines, management of peer review, and oversight and integration across center boundaries should remain Headquarters functions. Likewise, creation of a strategy and plan for the technologies that support space science should be a Headquarters responsibility. The adoption of industrial funding will further emphasize the importance of a suitably strong Headquarters organization.

Research in New Fields—The committee recognizes the competitive obstacles faced by smaller, newer, or less well-established fields of science. The committee recommends that NASA science budgets include dedicated funding for innovative, high-risk, high-return ideas falling outside current frameworks of inquiry or design. This research is highly important and deserves special management attention, including that of the Chief Scientist. This recommendation is not intended to allow circumventing of peer review for the major parts of any science program.

Science Institutes—Creation of contractor-operated institutes may be advantageous in specific instances. However, the committee recommends that, as NASA proceeds with arrangements for the first focused science institutes, it give due attention to the processes by which these institutes are selected and created, and by which, over a few years, their guaranteed base funding will be transformed into competed programmatic funding. Further, there should be consideration of a review process that will ensure either (1) that they compete successfully to maintain or increase their size, or (2) if less successful, that they are phased down in an orderly fashion. The committee recommends that additional initiatives along these lines be deferred until the above processes have been defined and the success of the two proposed institute pilots can be evaluated.

The committee's recommendations are gathered together by main theme in Chapter 7.

The NASA space science programs, from the dawn of the space age to the present, have produced an unprecedented flow of discoveries. The fiscal, political, and technological environment of the agency is now in a state of rapid change. It is vital that NASA respond to its challenges and opportunities in the most constructive manner to ensure the success of its future space science endeavors. The committee believes that the recommendations made in this report, if accepted by NASA, will aid in this objective.

⁴Federal Laboratory Review Task Force, NASA Advisory Council, *NASA Federal Laboratory Review*, February 1995.

3.7 The Role of Small Missions in Planetary and Lunar Exploration

A Report of the Committee on Planetary and Lunar Exploration¹

EXECUTIVE SUMMARY

The last 30 years have seen remarkable progress in our understanding of the solar system and its diverse constituents. But this period has also seen an upheaval in the political and economic circumstances that have been among the prime drivers of planetary and lunar exploration. The motives that led the United States, the former Soviet Union, and, to a lesser extent, various European nations and Japan to explore the solar system during the past three decades were political as well as scientific. Now, with the end of the Cold War, the political motive has virtually disappeared. With such strong roots in the former East-West confrontation, the space program in general and planetary exploration in particular have become vulnerable to changing national priorities. Some observers question the utility of a space program as an instrument of national policy, and others point to the nation's altered economic fortunes and ask if space exploration is a luxury we can no longer afford.

Against this backdrop, the past successes of the planetary exploration program are, paradoxically, endangering its future vitality. Telescopic observations combined with the Apollo lunar landings and a string of highly successful robotic missions, including Vikings, Magellan, and the Voyagers, have given us a first-order understanding of all the planets and major satellites in the solar system from Sun-scorched Mercury to frigid Neptune; even Pluto's gross characteristics are known from ground-based and Earth-orbital measurements. Thus we have finished the preliminary reconnaissance of the major bodies in the solar system and have entered an era of intensive study of the physical phenomena that shape our planetary neighbors. Increased knowledge and comprehension lead us to pose more fundamental questions requiring increasingly sophisticated and expensive investigations to answer. Thus—quite naturally—the small, simple, and inexpensive spacecraft sufficient 20 to 30 years ago to record basic data about the planets have given way to multibillion-dollar robots capable of performing multidisciplinary investigations in the farthest reaches of the solar system.

But the increased scale and scope of planetary missions have a cost other than that measured in dollars. With a planetary program composed only of a few large missions, each spaceflight becomes precious. This is especially true in an environment of declining status and budgets for space exploration, where the failure of any given mission is no longer tolerable. A result is engineering conservatism, with engineers forced to seek the “perfect” design. At the same time, in a program of few spaceflights, scientists—fearing that no other missions will fly soon—will attempt to take the maximum advantage of available opportunities and potential gains from synergistic measurements, something that could, uncharitably, be interpreted as “trying to cram as much aboard as possible.”

As we have slowly come to understand, deep-space missions are inherently difficult. Thus it is impossible to ever reduce the risk of failure to zero. With a space program built on occasional comprehensive missions, a simple mechanical failure (as with Galileo), or a breakdown with an uncertain cause (as with Mars Observer), or a budgetary problem (as with the Comet Rendezvous/Asteroid Flyby) can prematurely end—or at least seriously degrade—a large fraction of the nation's effort in planetary exploration. The most widely proposed solution to break this vicious cycle is to return to simpler, cheaper missions. With an assured, steady stream of small missions, occasional failures become, if not acceptable, at least tolerable.

Since early in the space program, NASA's astrophysics and space physics programs have built and flown a highly effective series of Explorer spacecraft. These low- to moderate-cost missions have transmitted a virtually continuous stream of important scientific data for more than three decades. NASA's earth science program recently instituted a similar series, the Earth Probes, to fill a comparable niche in its activities. Several attempts have been made over the last decade and a half to introduce a comparable line of small planetary missions. For a variety of reasons, these efforts have all failed. Undaunted, NASA recently proposed again to begin such a program, now called Discovery. Two small planetary missions, the Near-Earth Asteroid Rendezvous (NEAR) and Mars Pathfinder, received new starts in the FY 1994 budget as “Discovery” missions, although, as mentioned in the main report, they do not satisfy NASA's present guidelines for this program. Given this situation, the Space Studies Board charged its Committee on Planetary and Lunar Exploration (COMPLEX) to:

¹“Executive Summary” reprinted from *The Role of Small Missions in Planetary and Lunar Exploration*, National Academy Press, Washington, D.C., 1995, pp. 1-3.

1. Examine the degree to which small missions, such as those fitting within the constraints of the Discovery program, can achieve priority objectives in the lunar and planetary sciences;
2. Determine those characteristics, such as level of risk, flight rate, target mix, university involvement, technology development, management structure and procedures, and so on, that could allow a successful program;
3. Assess issues—such as instrument selection, mission operations, data analysis, and data archiving—to ensure the greatest scientific return from a particular mission, given a rapid development schedule and a tightly constrained budget; and
4. Review past programmatic attempts to establish small planetary science mission lines, including the Planetary Observers and Planetary Explorers, and consider the impact management practices have had on such programs.

In the course of its deliberations, COMPLEX found that rather than representing a fall from past glories, the initiation of a series of small missions presents the planetary science community with the opportunity to expand the scope of its activities and to develop the potential and inventiveness of its members in ways not possible within the confines of large, traditional programs. Some researchers may use the opportunities raised by a program of small missions to enhance or augment comprehensive studies of particularly interesting objects such as Mars and Jupiter. Others may employ them to perform reconnaissance of classes of relatively unknown objects such as comets and asteroids, to pursue aspects of intensive study of the terrestrial planets and the Moon, or to investigate planetary phenomena from Earth orbit. The rapid development schedules achievable with small missions should allow the possibility of exploiting targets of opportunity, should permit greater use of current technology, and should enhance the involvement of all sectors of the educational system in space research.

COMPLEX also realized, however, that a program of small planetary missions (such as Discovery) was, in and of itself, incapable of meeting all of the prime objectives contained in its report *An Integrated Strategy for the Planetary Sciences: 1995-2010*.² As explained in that report, a responsive planetary exploration program demands a mix of mission sizes ranging from comprehensive missions with multiple objectives (such as Galileo and Cassini) to small missions with highly constrained scientific objectives.

For a program of small planetary missions to fulfill its promise, COMPLEX believes that it must satisfy certain criteria. These include the following:

1. A continuing budget line should be initiated that is dedicated to a series of small planetary missions that focus on specific, well-defined objectives and are capable of yielding significant scientific results. The chosen missions should address key scientific questions and objectives as outlined in the report *An Integrated Strategy for the Planetary Sciences: 1995-2010*.
2. This budget line for small planetary missions should be funded at a level that will permit the launch of at least one mission per year, with approximately half of the accepted missions supported at a level close to the currently announced budget cap of \$150 million (FY 1992 dollars), not including inflation.
3. Each mission must be selected through open competition from proposals presented as an integrated package by a principal investigator. This individual should have full authority to decide the appropriate balance among science performance, mission design, and acceptable risk.
4. NASA should not impose on mission design arbitrary constraints such as preselection of launch vehicle, spacecraft bus, payload, or data rate, nor should NASA specify a particular management structure or a specific institution to run mission operations.
5. The budget, schedule, and risk envelope must be identified in the conceptual and definition phase of mission planning. It is essential for NASA to adhere to the agreed-upon funding profile.
6. Past NASA practices must change in order to foster the development of a streamlined approach to management of each complete mission.
7. As soon as they have been calibrated and validated, data and all subsidiary information (e.g., spacecraft ephemerides) needed for their interpretation should be archived expeditiously to ensure their prompt availability to the entire research community.
8. NASA's Planetary Instrument Definition and Development Program should be augmented to produce highly capable science instruments that are appropriate for use in the Discovery program.
9. The option of using elements of the small-mission philosophy for Mars Surveyor and future large missions should be studied.

²Space Studies Board, *An Integrated Strategy for the Planetary Sciences: 1995-2010*, National Academy of Sciences, Washington, D.C., 1994.

3.8 Setting Priorities for Space Research: An Experiment in Methodology

A Report of the Task Group on Priorities in Space Research¹

EXECUTIVE SUMMARY

In 1989, the Space Studies Board created the Task Group on Priorities in Space Research to determine whether scientists should take a role in recommending priorities for long-term space research initiatives and, if so, to analyze the priority-setting problem in this context and develop a method by which such priorities could be established. After answering the first question in the affirmative in a previous report, *Setting Priorities for Space Research—Opportunities and Imperatives* (National Academy Press, Washington, D.C., 1992), the task group set out to accomplish the second task.

The basic assumption in developing a priority-setting process is that a reasoned and structured approach for ordering competing initiatives will yield better results than other ways of proceeding. The task group proceeded from the principle that the central criterion for evaluating a research initiative must be its scientific merit—the value of the initiative to the proposing discipline and to science generally. But because space research initiatives are supported by public funds, other key criteria include the expected contribution to national goals (including the enhancement of human understanding), the cost of the initiative, and the likelihood of success. To be effective, a priority-setting process must also reflect the values of the organizations using it, be sensitive to political and social contexts, be efficient, and provide a useful product. The key elements of such a process include determining the categories of candidate initiatives that will be considered, specifying explicitly the criteria that will be used to evaluate initiatives, providing a mechanism for advocacy of initiatives, and providing a mechanism for evaluating the initiatives. Evaluation schemes can range from informal, subjective approaches to formal, quantitative methods. This general outline of a priority-setting process was expanded by the task group into a specific schematic sequence of distinct steps for selecting and ranking initiatives. The task group developed a two-stage methodology for priority setting and constructed a procedure and format to support the methodology. The first of two instruments developed was a standard format for structuring proposals for space research initiatives. The second instrument was a formal, semiquantitative appraisal procedure for evaluating competing proposals.

To help guide the development of the priority-setting process and instruments, the task group conducted two trials of preliminary versions. In the first, a small informal group of practicing scientists was convened at a workshop to evaluate a set of strawman initiative proposals prepared with the help of Board discipline committees. The results of this trial were used to refine the instruments. In a second trial, the Board itself exercised revised versions of the instruments and assessed their utility. The Board concluded that the method was not fully suitable for adoption on an operational basis. Reasons given by individual Board members for not supporting the proposed process included philosophical differences with the scope or approach of the method, reservations about the instruments themselves, and concerns about the ability of the Board and its committees to successfully implement them. Notwithstanding the reluctance of some members to adopt the proposed methodology, the Board remained broadly in accord with the task group's earlier finding that researchers should participate actively in priority setting.

This report makes available complete templates for the methodology, including the advocacy statement and evaluation forms, as well as an 11-step schema for a priority-setting process. From the beginning of its work, the task group was mindful that the issue of priority setting increasingly pervades all of federally supported science and that its work would have implications extending beyond space research. Thus, although the present report makes no recommendations for action by NASA or other government agencies, it provides the results of the task group's work for the use of others who may study priority-setting procedures or take up the challenge of implementing them in the future.

¹"Executive Summary" reprinted from *Setting Priorities for Space Research: An Experiment in Methodology*, National Academy Press, Washington, D.C., 1995, pp. 1-2.

3.9 A Science Strategy for Space Physics

A Report of the Federated Committee on Solar and Space Physics, Space Studies Board,
and Committee on Solar-Terrestrial Research, Board on Atmospheric Sciences and Climate¹

SUMMARY

This report by the Committee on Solar and Space Physics and the Committee on Solar-Terrestrial Research recommends the major directions for scientific research in space physics for the coming decade. As a field of science, space physics has passed through the stage of simply looking to see what is out beyond Earth's atmosphere. It has become a "hard" science, focusing on understanding the fundamental interactions between charged particles, electromagnetic fields, and gases in the natural laboratory consisting of the galaxy, the Sun, the heliosphere, and planetary magnetospheres, ionospheres, and upper atmospheres. The motivation for space physics research goes far beyond basic physics and intellectual curiosity, however, because long-term variations in the brightness of the Sun vitally affect the habitability of the Earth, while sudden rearrangements of magnetic fields above the solar surface can have profound effects on the delicate balance of the forces that shape our environment in space and on the human technology that is sensitive to that balance.

The several subfields of space physics share the following objectives:

- To understand the fundamental laws or processes of nature as they apply to space plasmas and rarefied gases both on the microscale and in the larger, complex systems that constitute the domain of space physics;
- To understand the links between changes in the Sun and the resulting effects at the Earth, with the eventual goal of predicting the significant effects on the terrestrial environment; and
- To continue the exploration and description of the plasmas and rarefied gases in the solar system.

Significant progress has been made in the more than three-decade history of space research. Many space plasma and rarefied gas phenomena have been characterized and are well understood, but many others are still under investigation and new discoveries continue to be made. Space physics asks fundamental questions about how plasmas are energized; about how the energy is redistributed with the result that a few particles are taken out of a near-thermal distribution and accelerated to superthermal or very high energies; about the specific roles played by magnetic fields in transferring energy from plasmas to particles and vice versa; about instabilities and interactions between waves and particles in a plasma; about the generation of magnetic fields through convection and rotation; and about the complex physical processes that operate in boundary layers between regions of different types of plasmas and rarefied gases. Some plasma configurations or particle distributions are known to be unstable and to relax spontaneously to a more stable state with the release of free energy, but there are many others for which the instabilities and wave-particle interactions are not yet understood. Determining the physics of such relaxation processes is fundamental to understanding and eventually being able to predict disturbances such as solar eruptions and geomagnetic storms, both of which can have important impacts on a technological society.

This strategy identifies five key scientific topics to be addressed in space physics research in the coming decade. For each of these topics, the report presents the scientific background and discusses why the topic is important, describes the current program for research on the topic, and then recommends, in priority order, research activities for the future. As is made clear in the main text, each of these five diverse topics is linked by a number of basic themes. Even though this strategy does not address specific proposals for future programs or missions, consideration is given to the practical aspects of carrying out the recommended investigations. The rationale for the research priorities is driven not only by scientific priority, but also by considerations such as current plans, near-term budget constraints, technological readiness, and balance between large- and small-scale endeavors. The five topics [box omitted], which are not prioritized, and the prioritized recommendations for research in each topic are briefly summarized as follows.

¹"Summary" reprinted from *A Science Strategy for Space Physics*, National Academy of Sciences, Washington, D.C., 1995, pp. 1-7.

THE KEY TOPICS IN SPACE PHYSICS RESEARCH

Mechanisms of Solar Variability

The Sun is a variable star on time scales of milliseconds to centuries or more. These variations occur not only in visible light, but also at radio, ultraviolet, x-ray, and γ -ray wavelengths and in the emission of the solar wind and energetic particles. Although solar variability ultimately arises from the interaction of magnetic fields and differential rotation inside the Sun, the Sun's interior dynamics are largely unknown. The new tool of helioseismology is being used to probe the solar interior; it has shown that the Sun's rotation rate does not increase inward as had previously been postulated and thus rules out the "standard" model of the dynamo that generates the solar magnetic field. At least at the solar surface, and perhaps in the interior as well, the magnetic fields are confined to rope-like structures with diameters of about 100 km, which are too small to be resolved from Earth. It has been suggested that the twisting and shearing of these flux tubes lead to bursts of high-speed solar wind, called coronal mass ejections, and to solar flares, but the trigger mechanisms for those violent events are not yet known.

On longer time scales, complacency about the simplicity of the Sun has been upset by the discovery and documentation in the historical record that the Sun has undergone periods of low activity. The association of the Maunder Minimum period (~1645 to 1715), when few sunspots appeared on the solar surface for roughly 70 years, with the Little Ice Age, when Europe experienced exceptionally cold winters, has potentially serious implications for society should a similar episode occur under current conditions. In addition, confidence in current understanding of the solar interior was upset by the discordantly low flux of solar neutrinos observed in several experiments. It is not yet known whether this disagreement derives from errors in neutrino physics or from errors in understanding of the solar interior.

The research priorities for advancing the understanding of solar variability are as follows:

- Use helioseismology to study the structure and dynamics of the solar surface and interior over a full solar cycle, to obtain information on the interior changes that cause solar cycles.
- Assure continuity of total and spectral irradiance measurements, supported by spatially resolved spectrophotometry, to investigate correlations between solar magnetic activity and solar output variations and thereby to understand how they are coupled.
- Measure high-energy radiation and particles from flares and coronal mass ejections with good angular resolution, good spectral resolution, and wide spectral coverage to determine what drives each of those phenomena and how they contribute to the solar output at high energies.
- Observe surface magnetic fields, velocities, and thermodynamic properties with enough spatial resolution (<150 km, with an ultimate goal of <100 km) to study small-scale structures such as flux tubes that may play a decisive role in solar activity and the generation of solar outputs.
- Make global-perspective measurements of the solar surface magnetic and velocity fields and solar oscillations to measure the three-dimensional structure and long-term evolution of active regions and to detect weak but coherent global oscillations.
- Measure active regions with angular resolution of ~1 arc sec and temporal resolution of ~10 min for a duration of ~10 days without nighttime gaps to determine the magnetohydrodynamic history of their emergence, development, and decay and the physical scenario behind it.

The Physics of the Solar Wind and the Heliosphere

Some of the energy transported from the solar interior goes into heating the Sun's outer atmosphere, called the corona, to over a million degrees by processes that are currently the target of intense study. The hot corona in turn becomes the source of the solar wind, but there are still major questions about how this occurs. Further observations and numerical simulations are required to determine the relative importance of magnetic reconnection, explosive jets, tiny active regions called bright points, hydromagnetic waves, and the topology of the magnetic fields in the corona in accelerating the quasi-stationary solar wind. There are additional questions about the acceleration of the nonstationary or transient solar wind arising from explosive events called coronal mass ejections. New observations of the variable elemental composition and ion charge states of the solar-wind plasma are providing valuable clues concerning the acceleration of both types of solar wind.

As the solar wind flows out through the solar system, it blows a big bubble, called the heliosphere, in the interstellar medium. Because of its very large scale (~100 AU), the heliosphere provides a unique laboratory for studying plasma processes in relative isolation from boundary effects; from heliospheric studies it is possible to learn much about instabilities in expanding plasmas, the interaction of colliding plasmas, the generation and evolution of plasma waves and magnetohydrodynamic turbulence, and the acceleration and propagation of energetic particles in turbulent fields. Within the decadal time frame considered by this report, it will be possible to measure the latitudinal variations of heliospheric particles and fields over the full range of solar activity and to test theories about the interaction of the solar wind with the interstellar gas and plasma.

The following priorities are identified for future research on the solar wind and the heliosphere:

- Continue to obtain and synthesize the data from the present constellation of heliospheric spacecraft and from the interplanetary cruise phases of planetary missions in order to characterize the global and solar-cycle-dependent properties of the heliosphere and its interactions with the interstellar medium.
- Carry out in situ observations of the solar corona to explore and characterize the region of acceleration of the solar wind and the physical processes responsible for that acceleration.
- Obtain new types of data required to reveal the mechanisms responsible for the transport of energy, including wave motions (periods of 1 to 10 s), from the solar surface into the chromosphere and corona to understand how these are heated.
- Carry out stereo imaging of the solar corona to reveal the three-dimensional structure of coronal features without the ambiguity caused by integration along the line of sight.
- Develop and use techniques for the remote sensing of the coronal magnetic field in order to improve knowledge of the acceleration of the solar wind and of the initiation of coronal mass ejections.
- Make in situ measurements of the outer heliospheric boundaries and the interstellar medium with instruments specifically designed for those purposes.

The Structure and Dynamics of Magnetospheres and Their Coupling to Adjacent Regions

Some of the most visually awe-inspiring, yet poorly understood, terrestrial phenomena are a direct consequence of the interaction of the variable Sun and solar wind with the Earth. Auroral displays, usually confined to high latitudes, episodically descend into the temperate zones during periods of extreme solar activity. The aurora is only one manifestation of the complex chain of physical events and connections that link the energy output of the Sun with the Earth's magnetosphere, ionosphere, and atmosphere.

As the solar wind reaches the Earth, some of it enters the magnetosphere via several different processes and paths and affects the circulation and dynamics of the plasma within the magnetosphere. The interplanetary magnetic field can become temporarily connected to the geomagnetic field, but the physics of the reconnection process is not yet well understood. Once within the magnetosphere, the energy from the solar wind cascades through the system and some is released catastrophically in events whose trigger mechanisms and extent are not well known. Flows of thermal and energetic plasma, large-scale current systems, magnetic perturbations, and imposed electric fields provide the basic links between the magnetosphere and the ionosphere. The ionosphere provides feedback to the magnetosphere in the form of ion outflow, conductivity changes, and dynamo fields. There is a continual reconfiguration of this system as the solar wind and its embedded magnetic field change in response to solar and interplanetary dynamics and energetics.

The past and current program, based primarily on in situ measurements, is providing an understanding of the magnetosphere that is strong in terms of local phenomena and a statistical picture of the global structure, but weak in terms of global dynamics. Researchers now know that most transport processes take place within narrow boundary layers connecting regions with very different plasma conditions. The frontier issues for the future center on the global magnetospheric dynamics in response to the solar wind driver, and the physical mechanisms that determine the coupling between regions. Many of the outstanding questions in magnetospheric physics will be addressed by global magnetospheric imaging, a new addition to the techniques available for magnetospheric research. In addition, studies of the magnetospheric environments of other planetary bodies can also yield important physical insights into the mechanisms that drive the dynamical behavior of the Earth's magnetosphere.

The following priorities are identified for future progress on this topic:

- Reap the full scientific potential of the International Solar-Terrestrial Physics program and its coordinated programs to advance understanding of the transport of mass, momentum, and energy throughout the solar wind, and the magnetosphere and ionosphere systems.
- Simultaneously image the plasma and energetic particle populations in the aurora, plasmasphere, ring current, and inner plasma sheet to study the global structure and large-scale interactions of magnetospheric and ionospheric regions during different levels of solar and geomagnetic activity.
- Maintain the full complement of particle and field instruments on current and future planetary missions to gain increased understanding of the formation and dynamics of diverse magnetospheres and ionospheres.
- Further develop and exploit ground-based facilities that image the ionospheric manifestations or “foot-points” of solar wind/magnetosphere coupling processes to complement the magnetospheric imaging initiative aimed at studying the global properties of the magnetosphere.
- Explore Mercury’s magnetosphere to understand the role of an ionosphere in coupling between the solar wind and planetary magnetospheres.
- Target localized regions that require greater understanding of the small-scale physical processes occurring there with high-resolution, multispacecraft measurements that take advantage of new smaller, lighter, more capable instruments and more sophisticated data-compression schemes.

The Middle and Upper Atmospheres and Their Coupling to Regions Above and Below

A complex interface exists between Earth’s space environment and the lower atmosphere or troposphere where weather and climate occur. This interface includes the highly variable middle and upper regions of the atmosphere extending upward from a lower boundary at 10 to 15 km altitude. The middle and upper atmospheres have considerable practical as well as intellectual interest because most ozone resides there and because disturbances in the upper atmosphere and ionosphere caused by solar wind and magnetospheric variations can disrupt technological systems through their effects on satellite drag, communications, and induced ground currents.

The middle and upper atmospheres are strongly influenced by inputs of mass, momentum, and energy from both above and below. The absorption of variable solar ultraviolet and x-radiation and of energetic particles not only heats the atmosphere, but also initiates chains of photochemical reactions and ionizes the upper atmosphere to form the ionosphere. Highly variable electric fields and currents originating above and below the upper atmosphere are major sources of energy and momentum to that region. Gravity, planetary, and tidal waves that originate partly from the lower atmosphere grow in amplitude as they propagate upward, where they contribute to the momentum and energy budgets of the middle and upper atmospheres and produce turbulence that influences mixing processes. There are major deficiencies in our knowledge of many of these inputs to the middle and upper atmospheres as well as of the multiple interactions and feedbacks that occur there.

The following priorities are identified for future research aimed at understanding this important interface between Earth’s lower atmosphere and space:

- Exploit the exciting new capabilities of UARS,² FAST, and CEDAR to provide the foundation for future advances in our understanding of the middle and upper atmospheres.
- Investigate the reaction of the middle and upper atmospheres to upward propagating waves from the lower atmosphere and energy inputs from space so that the sources of important features such as the quasi-biennial and semiannual oscillations and the causes of mesosphere/lower-thermosphere structure and variability can be understood.
- Study the long-term variations in the middle and upper atmospheres using a combination of consistent long-term satellite and ground-based measurements together with three-dimensional radiative-chemical-dynamical modeling to understand natural and anthropogenic changes in these regions.
- Develop methods to observe the time-dependent electrodynamics operating on microscales to global scales, both in the upper atmosphere-ionosphere-magnetosphere coupling regions so that feedback processes can be characterized, and in the regions above thunderstorms so that the effects of electrified clouds on the “global circuit” and on middle atmosphere chemistry and energetics can be characterized.

²A glossary of acronyms is included as the appendix, and the principal programs identified by the acronyms are described in the main text.

- Take advantage of opportunities to include carefully chosen, appropriate instruments on planetary orbiter missions to make measurements critical to understanding planetary aeronomy and its relation to terrestrial aeronomic processes.

Plasma Processes That Accelerate Very Energetic Particles and Control Their Propagation

Many of the plasma processes responsible for phenomena within the heliosphere probably also play a role in determining the properties of galactic cosmic rays, which are the only available sample of matter from outside the local solar neighborhood. Mass, charge, and energy spectrometers on existing and planned spacecraft can make in situ measurements of energetic particles throughout the heliosphere to study particle acceleration, fractionation, and transport in a variety of space plasma environments. Theories of acceleration mechanisms in larger-scale galactic structures such as supernova remnants make specific predictions about compositional changes that should take place at the highest energies attainable in those objects, but the theories have not yet been tested observationally.

Cosmic rays are confined to the galaxy by turbulent magnetic fields. Measurements of radioactive “clock” nuclei can be used to distinguish diffusive trapping in the galactic halo from the simpler phenomenological models used for many years. Gamma ray measurements can be used to trace the radioactive parent elements of positrons that should be accelerated by the shocks presumed to be the source of the galactic cosmic ray nuclei. Although fluxes of antiprotons and positrons produced in collisions of cosmic rays with gas in the interstellar medium can be calculated with some precision, a full understanding of the sources of antimatter in the cosmic radiation requires a new generation of measurements.

Nuclides heavier than nickel are produced by accretion of neutrons either in supernova explosions or during certain other phases of stellar evolution. Knowledge of the abundance of different cosmic ray elements and isotopes will allow the use of nucleosynthesis models to determine quantitatively the fraction of cosmic rays synthesized in each type of source. The abundance of actinide elements can be used as a radioactive clock to determine the time delay between the synthesis of these elements and their acceleration to cosmic ray energies.

The measurements necessary to address scientific issues concerning particle acceleration and propagation are, in priority order:

- Complete the observations from the current and planned network of interplanetary spacecraft to study particle acceleration, fractionation, and transport.
- Extend direct composition measurements to 10^{15} eV to probe the limits of acceleration and trapping mechanisms.
- Measure abundances of radioactive isotopes above 1 GeV/nucleon to search for evidence of an extended galactic magnetosphere and wind.
- Measure the spectra of positrons (10 MeV to 100 GeV) and antiprotons (100 MeV to 100 GeV) to determine where those particles are created and how they are accelerated.
- Measure isotope abundances for nuclei heavier than nickel and elemental abundances through the actinides to probe the plasma regions where the nuclei are synthesized and to measure the time scales involved.

RECOMMENDED RESEARCH EMPHASES

The specific programs required to obtain answers to the questions raised under each of the five key topics outlined above are quite different. However, they are united by four common elements or themes that the CSSP and CSTR consider to be the most important research emphases for space physics in the next decade.

1. Complete currently approved programs.

The space physics community must reap the benefits of the existing approved programs. A stable program permits the most efficient management and execution of high-priority research. In addition to the obvious scientific return, these ongoing programs provide the basis for developing future research directions. Space physicists will gain the maximum benefit from ongoing and approved missions by:

- Completing the diverse components of the International Solar-Terrestrial Physics program;
- Enhancing data analysis and interpretation efforts;
- Streamlining mission operations for all space physics missions;
- Carrying out extended missions for the uniquely placed Voyager (to the greatest possible heliocentric distance) and Ulysses (through the solar polar passes at solar maximum);
- Supporting essential observational programs that require long-duration databases; and
- Enhancing the effectiveness of some of the longer-duration programs by soliciting new ideas and analysis techniques from guest investigators and by ensuring easy access to archived data resulting from the various programs for use in “small science” research tasks.

2. Exploit existing technologies and opportunities to obtain new results in a cost-effective manner.

Much technology is already in place to take the next observational steps required to address many of the important questions in space physics outlined here. These steps include:

- Adapting existing instrumentation to the new generation of smaller spacecraft and more focused space missions;
- Placing space physics instruments on planetary, Department of Defense, and other spacecraft of opportunity;
- Utilizing suborbital platforms such as rockets and long-duration balloons for both science objectives and instrument development; and
- Supporting, where appropriate, activities at unique ground sites such as in the polar cap.

3. Develop the new technology required to advance the frontiers of space physics.

In order to achieve several high-priority objectives, or to lower the cost of projects, the limits of technology must be pushed in the following ways:

- Developing methods to approach the Sun ever more closely to open one of the most exciting new frontiers of space science;
- Producing new spacecraft and instruments based on lightweight structure and miniature electronics;
- Extending capabilities in suborbital techniques for both experimentation and instrument development;
- Exploiting infrared instrumentation for solar physics; and
- Devising techniques to explore the region between the altitudes reached by balloons and those reached by spacecraft.

4. Support strongly the theory and modeling activities vital to space physics.

Special emphasis should be given to the following topics:

- Designing a new generation of instrumentation for remote global imaging of magnetospheric, ionospheric, and solar wind plasmas;
- Recognizing that synergy between observations, modeling, and theory provides the optimum way of addressing the principal questions in space physics;
- Making numerical simulations of space physics systems more realistic by extending them to three dimensions, longer time durations, and a greater range of scale sizes, and by incorporating additional physical and chemical processes;
- Ensuring access to state-of-the-art computational facilities;
- Exploiting new insights gained from theory, especially the theory of nonlinear processes; and
- Revisiting earlier efforts to predict solar activity, such as coronal mass ejections and flares, using simulations combined with solar observations.

4

Short Reports

4.1 On NASA Field Center Science and Scientists

In response to a request for guidance on the roles and mission of science and scientists at the NASA field centers, the Space Studies Board sent the following letter to NASA Chief Scientist France A. Cordova on March 29, 1995.

On behalf of my fellow Space Studies Board members, I would like to thank you for visiting with us on March 1 and for providing us with a broad discussion of the budget challenges facing NASA and of efforts under way to meet these challenges. You described NASA's urgent need to identify ways to reduce staff levels in order to meet the Administration's budget targets for future years. In particular, you described the process by which NASA senior management is exploring possible consolidations, redistributions, and reductions of science activities at NASA Headquarters and at the field centers.

We subsequently pursued some of the issues you raised in conversations with Associate Administrators W. Huntress, H. Holloway, C. Kennel, and A. Ladwig. We also had the opportunity to discuss them during several intervals, including Executive Session periods, at our meeting and during a subsequent teleconference of our Executive Committee.

During your visit, you requested a rapid response from the Space Studies Board to help you and other senior managers identify key principles to be considered for preserving or even strengthening NASA's ability to carry out its goals in space research as you continue to explore downsizing options. Your interests were further clarified in your memorandum to me, dated March 9, 1995, which specifies two issues on which NASA seeks comments from the Board:

1. The roles and mission of NASA center scientists, as they enable the national resource of space science; and
2. Alternative management models for the science enterprise.

In this letter we briefly present our observations regarding these issues. The urgency of your schedule, which requires a major management decision by mid-May, 1995, does not permit a more exhaustive study. Nonetheless, we hope these limited observations will be of some assistance.

In its discussions, the Board proceeded from the premise that science will continue to play an essential role in NASA, as it has during the nearly four decades of the agency's existence and as called for in the Space Act. The most recent NASA-wide strategic plan strongly reasserts the centrality of science to NASA; the three science offices span three of the five major NASA enterprises and arguably contribute to the others as well. At the same

time, we are mindful of the rapid evolution of the conduct of space science in NASA and note that reorganization, though painful, could provide an opportunity to strengthen the agency's ability to function in new ways.

1. Roles and mission of scientists within NASA

Before elaborating the functional roles of NASA scientists, we stress two points. First, we believe that the most important mission of NASA scientists is to bind NASA's immense engineering and technical capabilities to the still larger and more diverse industrial and academic research communities across the country and around the world. Without such a tight binding, NASA cannot remain at the forefront of science, nor can these broad and diverse communities make the most effective and scientifically productive contribution to and use of the nation's civilian space infrastructure. While it may take new forms, a close coupling between the agency and the spectrum of research communities will become even more critical in a new, leaner NASA, with its increased emphasis on NASA-university-industry partnerships like the Discovery program, long-lived, multicomponent research activities like the Earth Observing System, and multiuse orbiting research facilities like Spacelab and the International Space Station.

Second, we believe that this binding requires that NASA have world-class scientists who, as a group, combine both the internal and external functional roles described below and are themselves sufficiently tightly integrated into NASA's engineering and technical infrastructure. The very fact that NASA's scientists serve both internal and external roles establishes a conduit between NASA and the research community. At the same time, these scientists must conduct their own independent scientific research at the frontiers of their disciplines in order to remain world-class. Such research is, therefore, itself another essential mission of NASA's scientists.

The specific functional roles of NASA scientists are associated with their clear mission of enabling the space science activities of the agency. These roles can be classified as internal, supporting the conduct of programs within NASA, or external, interacting with the broader research community. We believe that both kinds of roles have been and will continue to be of critical importance.

Examples of important internal functional roles of NASA scientists include:

- Providing scientific leadership and expertise to support formulation of NASA policy and management of the agency;
- Providing the scientific component of implementation oversight for space science missions during development and operations phases;
- Providing direct and responsive scientific expertise for the definition, design, development, and operations of space assets and of supporting ground assets;
- Assuring the scientific quality and utility of NASA facilities in space and on the ground;
- Initiating and developing enabling technology and innovative instrumentation for space science through synergy with engineers and technologists; and
- Providing direct and responsive scientific expertise in the specification and oversight of NASA contracts and grants.

Examples of important external functional roles of NASA scientists include:

- Conducting and overseeing selection of investigations and investigators, peer reviews, and advisory committees;
- Providing interfaces and facilitating interactions between extramural investigators and NASA's technical capabilities and infrastructure in space and on the ground;
- Fostering new, interdisciplinary or multidisciplinary scientific research made possible by the unique opportunities offered by the space environment or space missions and by special supporting facilities and research assets at NASA's field centers; and
- Providing both outreach to, and in-reach from, the scientific community, the educational community, and the public for space research, one of NASA's most visible and widely accessible activities.

2. Alternative management models for the science enterprise

As you note in your memorandum, the Board has undertaken the Future of Space Science (FOSS) project, which includes an in-depth study of the broad question of alternative organizations for science in NASA. The Board task group charged with the organizational portion of the study is now only part way through a systematic assessment and is not, therefore, in a position to issue a meaningful report in time for the May deadline.

As part of the recent Board discussion, however, we did consider the question of what fundamental principles should help define the roles of science and scientists in NASA. These principles, in effect, derive from the “roles and missions,” above. They may be of help in evaluating alternative ways of managing NASA science.

If the most important mission of NASA scientists is to bind NASA to the broader research communities, then the most fundamental principle is to assure that this binding is maintained or even strengthened through any reorganization. This principle underlies many of the following more specific ones:

- Research quality should be excellent. Whatever role science assumes in NASA, there must be an uncompromising commitment to the highest standards. Maintaining excellence is essential for the effective discharge of both the internal and external roles described above. To be excellent, NASA scientists must, as a rule, engage in frontier research secured in open and fair competition with outside investigators, through selection based on uniform peer review. Exceptions for programmatic research or incubation of new ideas should be limited in scope and duration.
- NASA should maintain sufficient *breadth* of scientific activity to maintain connections to all the major disciplines involved in NASA’s research program. Not all subdisciplines need be present within NASA, nor is this feasible. But every external subdiscipline relevant to NASA’s research program should have a clear and natural connection to some part of the agency. Scientists who individually have broad or multidisciplinary talents or who represent emerging disciplines of interest to the agency have special value in this regard.
- NASA should also maintain appropriate *depth* in its science groups to maintain excellence. At one extreme, there must be at least a “critical mass” of collocated investigators in a subject to provide a productive, stimulating research environment. At the other extreme, center staffing in a discipline that greatly exceeds this critical mass may tilt the balance away from university research during a time of decreased resources.
- NASA science should be firmly integrated into the NASA infrastructure. Effective coordination of scientific research needs with technical and engineering capability is difficult to achieve and fragile because of the inevitable tensions between the two “cultures” of basic science and practical engineering. When these cultures work together, the resulting synergy yields spectacular successes, as NASA’s history attests. But this coordination requires continual nurturing, and cannot be maintained at arm’s length. Therefore, in addition to the essential need to have cognizant scientists at a center implementing a particular major research program, it is advantageous to strategically distribute science activities across the agency. Counter-arguments for greater consolidation arise from the desire for administrative efficiency and from the scientists’ own need to maintain a “critical mass” at any one location. These competing considerations should be carefully balanced in making any changes that might prove difficult to reverse.
- NASA should strengthen its sense of interdependency with the broader research communities. The need to achieve research quality through scientific competition has the danger of creating conflicts of interest and instincts of self-preservation at NASA centers. Scientists at NASA Headquarters have played an essential role in mitigating these negative tendencies in the setting of policy, the conduct of peer reviews, and the implementation of programs. As Headquarters staffing is reduced, this role must be maintained. Moreover, NASA should strive to assure that the centers themselves and their senior managers assume greater responsibility for a healthy partnership with the external industrial and university community. Formation of substantive partnerships across NASA and between NASA and external institutions is just one example of a way to foster a sense of interdependency. Another example at the working level is the actual cycling of working scientists around NASA, into NASA from outside institutions, and from NASA to outside institutions (through leaves or sabbaticals).

The Board believes that these principles also apply to alternative organizational arrangements designed to carry out some of the scientific functions noted above but managed for NASA by nonprofit institutions like universities or by another (remote) center. The space program itself has many examples of alternative management approaches, for example the Jet Propulsion Laboratory managed by the California Institute of Technology, the Applied Physics

Laboratory managed by the Johns Hopkins University, and the Space Telescope Science Institute managed by the Association of Universities for Research in Astronomy. An assessment of the strengths and shortcomings of these and other management approaches could provide guidance for NASA as it strives to streamline its organizations and operations.

The Board recognizes that sweeping changes are in store for NASA and its science programs. The final results of the FOSS study, now in progress, will address many of the above issues in more depth and detail. We are confident that NASA can continue to provide the nation excellent value in science, technology, and inspiration, building on its solid record of achievement. We look forward to continuing to work with you to assure an optimum return on the nation's space research investment in the years ahead.

*Signed by
Claude R. Canizares
Chair, Space Studies Board*

4.2 On a Scientific Assessment for a Third Flight of the Shuttle Radar Laboratory

On April 4, 1995, the Space Studies Board and the Committee on Earth Studies sent the following letter to NASA Mission to Planet Earth Associate Administrator Charles Kennel.

The Committee on Earth Studies of the Space Studies Board held a workshop at the Beckman Center from January 9th to January 11th to begin the study of spaceborne synthetic aperture radar (SAR) systems that you requested. The workshop was preceded by an extensive data-gathering phase that your staff performed with guidance from us as to our needs. It proved convenient to divide the data gathering into the categories of ecology, ice sheets and glaciers, oceanography, hydrology, solid earth, and technology. Your staff enthusiastically took on a difficult task in a compressed time frame, and they are certainly to be commended. You have also requested an early scientific assessment prior to the completion of the overall study to guide your decisions and/or planning for a third flight of the Shuttle Radar Laboratory (SRL); this letter provides that assessment.

Beyond this brief science assessment, the overall results of the study you have requested of spaceborne radar systems must await the completion of the final report at the end of the study. We begin with a few general comments.

Overview Comments

The use of SAR for civil research and operational applications has been advanced by the series of Shuttle-based SAR flights (SIR-A, SIR-B, and the U.S.-Germany-Italy SIR-C/X-SAR) and the European Space Agency's ERS-1. Some contributions have also been made by the Russian Almaz-1 and Japan's JERS-1. NASA's aircraft-based experimental system, AIRSAR, has played a vital role in complementing and enhancing the understanding of the space-based measurements, as have systems developed in Germany (E-SAR) and the Netherlands (PHARUS). The accompanying research and analysis (R&A) program has also played an indispensable role in advancing the utility of this technology.

In spite of these commendable advances, there is no doubt that SAR systems remain less familiar and are less frequently employed than are more conventional electro-optical sensing systems. While both kinds of systems can be used to produce images of the Earth, the interpretation of the images is necessarily quite different between the two. As a result, the research and operational user communities have had a lengthier period to go through in learning how to use SAR data, and a major part of the learning has involved significant research in determining what the data show. That research continues. The moisture and frequency-dependent variable surface and vegetation penetration of microwaves, for example, certainly requires a reorientation of the thinking of image analysts. The problems of layover and shadowing also pose challenges in the interpretation of radar data. Lastly, until some of these issues are better understood, the research community cannot effectively include SAR data in processing algorithms that link near, short-wave, and long-wave infrared information.

At the same time, however, the additional learning the community has undergone can pay dividends. Electro-optic sensors, as powerful as they have become, are inherently limited by cloud-cover, fog, and dust—all of which may be persistent phenomena in some regions of the world, or which may be expected to accompany natural disasters. Indeed, in most regions of the world, one cannot rely on being able to obtain a surface image from an electro-optical sensor at the time the image is most needed. Because of their day-night, all-weather capability, microwave systems may represent the only reliable approach to collecting data on a given region at a particular time. In addition, unlike electro-optical systems, the signals returned by radar systems are sensitive to the physical structure and moisture content of the surface being sensed and may offer avenues to obtaining results that are important for research and applications but are not otherwise obtainable.

For all of the above reasons, there are some who believe with possible justification that, while radar imaging systems today play a secondary role to the electro-optical sensing systems, the role will be reversed in the future. Whether this "bullish" view proves to be correct or not is less important than is the acknowledgment that active microwave systems are demonstrating their worth, and that room exists for still further technological enhancement of their capabilities. Thus, although it is understandable why active microwave sensors have not occupied a more

prominent role in the early development of the planning for the Mission to Planet Earth, it should be expected that they will become increasingly important in the future—and likely be indispensable in some applications.

Putting aside for the moment the committee's generally favorable view of the long-term potential of SAR measurements for the Mission to Planet Earth, the committee recognizes that a major immediate issue that you are facing is deciding whether or not to seek a third flight of the Shuttle Radar Laboratory (SRL-3). Based only on scientific considerations, it is the committee's judgment that such a flight would produce good scientific results, if the current instrumentation were simply reflown, but that it would produce especially worthwhile results if it were modified for dual-antenna interferometric measurements of topography. Support for that view is provided below.

It is important, however, to note that the committee has no view or expertise on the cost of a third flight, the feasibility of modifying the instrumentation to add a dual-antenna capability within a given schedule, or the realism of gaining a third flight in the Shuttle manifest. Your staff has provided us with some information on these matters, but the committee has no basis on which to evaluate trade-offs.

Even at this early stage in our deliberations, it is evident that the question of transitioning these results to an operational application is a complicated one, but also an important one. SAR is proving itself to be valuable; the community will not be content for it to remain in only a research status. In this regard, the committee has not yet examined the various orbit, coverage, and repeat-cycle issues that will be important in operational applications.

The next section of this letter addresses the individual scientific disciplines in slightly greater depth, with principal emphasis on what could be obtained from a third SRL flight.

Individual Disciplines

- Ecology

The committee notes that the data presented show that single-frequency, single-polarization SARs are sensitive to above-ground biomass differences in forests up to approximately 100 to 150 metric tons per hectare. Multichannel SAR systems that include low frequencies (L-band at 24-cm wavelength and P-band at 65-cm), and a higher frequency (C-band at 6-cm or X-band at 3-cm) can be used to estimate biomass levels up to 250 to 300 tons per hectare. This biomass range includes all forests except mature old-growth forests in temperate regions and some areas of tropical rain forests.

Because of their sensitivity to structural characteristics, multiparameter SAR systems offer a means to classify vegetation cover. It has been demonstrated that SAR data can be used to detect deforestation and forest regrowth and discriminate among up to ten distinct vegetation types in a region with accuracies comparable to data obtained with electro-optical remote sensing systems (i.e., approximately 89%). SAR is also sensitive to temporally dynamic factors such as moisture content and freeze/thaw status.

SAR appears particularly suited to detecting flooding in general and flooding under a wide range of vegetation cover in particular. For flooded forests, a lower-frequency (L- or P-band), HH-polarized SAR is required. For flooded herbaceous wetlands, a higher-frequency, HH- or VV-polarized SAR is better. The all-weather capabilities of SAR allow for repetitive coverage of flooded regions and provide a unique tool for use in disaster relief.

There remain a number of open questions. What temporally varying factors influence SAR signatures in the full range of vegetation and climatic regions worldwide? How sensitive is SAR to variations in the amounts of foliage in forests? What is the use of interferometric SAR in vegetated regions?

Were a mid-summer SRL-3 flight undertaken using the current equipment, it would enhance our understanding of the ability of multiparameter SAR to monitor Northern Hemisphere temperate crops and forests under full foliage conditions. The flight could enhance wetland delineation and mapping, and continue the analysis of forest regrowth. Collaboration with operational agencies could lead to an experimental test of the use of SAR in flood detection and relief planning.

Modifying the instrumentation to include the interferometer boom would allow the evaluation of the utility of mapping topography in vegetated terrains. It would offer an enhanced digital elevation model that could improve the mapping of vegetation cover and canopy characteristics in topographically complex terrains. The flight would also provide added information that could be used to explore whether additional data on land-surface characteristics are present in interferometric SAR data.

- Ice Sheets and Glaciers

Amplitude data alone permit the determination of snow facies, seasonal melt, surface morphology, ice velocity in rapidly moving regions, and iceberg production. The mapping of the snow facies of the Greenland ice sheet, for example, is a demonstrated capability that should be continued on an annual basis.

Multi-image complex data (amplitude and phase) add full spatial fields of ice velocity and surface topography. Interferometric SAR is the most important development for determining the surface velocity and topography of glaciers and ice sheets. Given suitable orbital parameters, interferometric SAR can provide a unique data set that cannot be obtained by any other means.

Single-frequency, single-polarization SAR will continue to contribute to research and operations. Multi-frequency SAR is required for probing the snowpack to different depths, but ascertaining the quantitative capabilities of these data requires further research. Snow-water equivalent cannot be measured for wet snow, because of the inability of the SAR signal to penetrate sufficiently into the snow. The snow-water equivalent of dry snow may be susceptible to measurement using multi-frequency polarimetric SAR, but this requires experimental verification.

An SRL-3 flight would expand the data set needed to answer many of the remaining questions regarding the utility of the L-band or X-band in multi-frequency and interferometric investigations of ice sheets and glaciers. However, the geographic coverage would not be large, as the 57° orbital inclination limitation does not permit measurements on the major ice sheets.

- Oceanography

In coastal oceanography, single-frequency single-polarization SARs have demonstrated the capability to observe internal waves, surface waves, bathymetric features, and the location of ocean fronts. In the open ocean, the frontal location of major currents such as the Gulf Stream and California Current can be measured. Multi-frequency, multi-polarization SAR has shown a capability to distinguish between oil spills and natural surfactants.

Although oceanography will be a significant element in the committee's overall SAR study, the committee could not make a compelling argument for it being a driver for the reflight of the SIR-C/X-SAR equipment on an SRL-3. Even adding the interferometric capability does not offer a great deal to oceanography in the form that the interferometer is usually conceived. If it were feasible to rotate one of a pair of interferometer antennas by 45°, then the SRL-3 flight could be used to test the concept of ocean surface velocity determination and surface wind velocity determination. Were these latter capabilities to prove successful, then the oceanographic community could become a stronger driver for future advanced SAR missions.

- Hydrology

Snow hydrology has already been discussed above. Soil moisture is a key variable in both research and operational applications. Aircraft, truck-mounted, and ERS-1 measurements have shown that surface soil moisture is correlated with radar backscatter. However, the nature of the correlation is strongly affected by surface roughness and slope and vegetation cover. The instrument responds to soil moisture in the top few centimeters, not the deeper soil moisture.

The earlier SRL flights did not provide the data necessary to assess the utility and desired parameters for a multi-parameter SAR system to measure soil moisture on a routine basis. The previous flights took place during seasons when the soil moisture was evenly distributed. A midsummer flight would offer the opportunity to continue the investigations at a more favorable time. Obviously, hydrology would be a major beneficiary of a modification of the SRL to produce a topographic map within its latitudes of coverage.

- Solid Earth

SAR has demonstrated its utility as an all-weather, geologic mapping tool that offers high spatial resolution. A steerable antenna provides rapid site revisit. Multi-frequency SAR is required for precision measurements to remove the effects of variable ionospheric delays. Even with multi-frequencies, however, the removal of artifacts due to the variable wet tropospheric delays requires ancillary ground-based observations (e.g., Global Positioning

System (GPS) observations). Multi-polarization data may facilitate lithologic discrimination, but their quantitative use has not been established.

The most compelling uses of SAR for solid earth studies involve interferometric SAR. A major achievement would be the construction of a global digital elevation model that is referred to a single, global geodetic reference system. Interferometric SAR has demonstrated the capability to image surface deformation at the millimeter level on regional scales. This capability would permit the measurement of large-scale topographic changes associated with earthquake cycles, small-scale topographic changes due to volcanic inflation/deflation, lava flows, erosion, human activities, migration of mobile geologic features (e.g., sand dunes and glaciers), and incipient landslides. However, the role of multi-polarization, multi-frequency SAR in this application is unclear at this time.

Regarding your question about the complementary nature of SAR and the GPS, detection of motion with SAR is complementary to GPS observations at point locations in several ways. One is the obvious continuous spatial imaging provided by SAR as opposed to the point positions obtained with the GPS. The second is that the GPS can provide full three-dimensional vector motion determinations, while SAR gives only displacements along the line of sight. A third is the continuous monitoring capability of the GPS, which permits the resolution of temporal variations of crustal motions in earthquake or volcanic eruption cycles.

An SRL-3 would allow the continuation of experiments to test the above possibilities. In its present form, the SIR-C/X-SAR equipment does not appear likely to add greatly to the science base. If the orbit is not an exact repeat of SRL-2, the value would be that of obtaining data from some new regions. If an exact repeat orbit is attained, there may be the opportunity for limited interferometric analyses using data from the two missions. On the other hand, modifying the mission to provide continuous interferometry would provide the digital elevation map mentioned above for the region from 57°N to 57°S latitude. In the opinion of key members of our committee, this could be one of the most useful NASA missions ever flown for geology and land-use studies.

Final Comments

The committee hopes that these initial observations are of assistance to you. In summary, the unmodified SRL equipment would permit useful, but nevertheless incremental, extensions of the previous results, while the addition of an interferometer boom would produce a new set of important data.

Please pass on to your staff our appreciation for their responsiveness and professionalism. The preparations for the workshop were exceedingly well done and greatly eased our task.

Signed by
Claude R. Canizares
Chair, Space Studies Board
and
John H. McElroy
Chair, Committee on Earth Studies

4.3 On Clarification of Issues in the *Opportunities* Report

On April 19, 1995, the Space Studies Board and the Committee on Microgravity Research sent the following letter to Mr. Robert Rhome, director of NASA's Microgravity Science and Applications Division.

In response to the questions you originally raised at its February 8, 1995, meeting, the Space Studies Board's Committee on Microgravity Research is pleased to offer clarification of the recommendations made in its report *Microgravity Research Opportunities for the 1990s*. The committee received your letter, dated February 27, 1995, in which you outlined several questions that were of greatest interest to you. The committee subsequently met on March 31, 1995, to finalize its response to questions posed in your letter. The questions and the accompanying committee responses are given below.

1. The Committee notes that although reproducibility of results is a critical element of laboratory science, nonetheless a balance should be established between reflight opportunities for reproducibility and the flight of experiments that address new scientific issues. Are there any decision rules or general criteria that NASA should apply to test whether we are meeting the intent of this likely recommendation?

Response: The requirement that experiments must incorporate new science in order to re-fly should not be a "decision rule." The committee acknowledges that hard and fast rules cannot be applied to reflight decisions and that the judgment and experience of Microgravity Science and Applications Division (MSAD) scientists and engineers must play key roles in striking a balance between reflight opportunities and new experiments. General criteria that the committee believes would be usefully applied in making these decisions include scientific importance, flight availability, competition from other experiments, and past experiment performance, all of which should be weighted heavily. The probability that the experiment will achieve its operational and scientific objectives is also an important consideration. This can be determined in part by evaluating the scientific maturity of the investigation, including the success of the ground-based investigation and the appropriateness of the theoretical modeling. However, this statement should not be construed as advocating a higher priority for investigations based on the length of their tenure in the microgravity program. Reflight of experiments should be subject to the same peer review criteria as any other experiment.

2. As there may be specific reasons to augment the microgravity research with a variable-g capability of extended duration, shared utilization of a centrifuge on the Space Station for microgravity research would appear to be desirable. If shared use were not possible, what relative priority would the Committee give to development of a unique centrifuge for this purpose over other hardware development programs already underway within NASA's microgravity research activities?

Response: A general-purpose variable-g centrifuge has a lower priority than other hardware development programs already under way within NASA's microgravity research program. The committee recognizes, however, that gravity as a variable is an important issue and that the development of special-purpose centrifuges may be justified in the future for specific experiments.

3. The Committee is aware of the importance to NASA of categorizing experiments according to their minimum facility requirements to maximize scientific return and cost-effectiveness. NASA MSAD would be very interested to learn from the Committee how to test for 'cost effectiveness' as NASA struggles to become "better, faster, cheaper."

Response: The *Opportunities* report points out that minimum platform facilities should be utilized where possible in the interest of lowering experiment costs. Although the role of cost-effectiveness in creating a "better, faster, cheaper" program is a legitimate and important issue, it is beyond the scope of this report. The committee believes that question is not answerable without considerable further study.

4. Tradeoffs must be evaluated when suggesting to principal investigators that general-purpose laboratory equipment, versus experiment-specific equipment, be used to support their scientific protocols. MSAD is aware that in seeking cost effectiveness there may be some degradation of scientific results and it would be helpful to hear how the Committee would expect MSAD to evaluate and/or reconcile these tradeoffs?

Response: The committee believes that it would be a mistake to restrict investigators to generic facilities. MSAD should continue to provide opportunities for the development of experiment-specific hardware—as well as access to generic facilities. However, since experiment costs for the former are expected to be significantly greater than for the latter during the space station era, it is reasonable to judge proposals for research requiring new hardware more rigorously than those for research utilizing facilities already in place. Investigators requesting the development of complex new hardware would therefore have to compete for more limited flight opportunities than would other investigators. This policy would need to be clearly stated in the NASA Research Announcements.

5. The Committee is aware that much of the extant NASA infrastructure and procedures were developed for missions in space with purposes other than laboratory science and that there has been an effort made in the past to simplify and unify the interactions among centers and between centers and NASA headquarters. MSAD has also worked very hard to ensure that principal investigators are continually involved with the development of experiments. MSAD believes that considerable progress has been made in these regards and would like to learn from the Committee if continuing concerns in this area are related to activities within the microgravity science research program (versus the way NASA does business, i.e., Spacelab from MSFC, mission from JSC and integration by KSC) and if these concerns are based on a community consensus or are more indicative of anecdotal exceptions to the improvement trend.

Response: The committee recognizes the considerable progress MSAD has made in the last few years in reducing the difficulties experienced by investigators interacting with NASA centers and their requirements. Room for further improvement still exists, however, and MSAD should remain vigilant on this issue. Opportunities remain for streamlining the diverse requirements imposed on investigators by the centers. Procedural requirements, particularly those pertaining to safety, are often applied across the board to experiments with very different needs and levels of risk. One possible improvement that MSAD might consider is to allow more flexibility in imposing NASA requirements on different experiments.

6. Since 1991, NASA's microgravity science research program has been pursuing the objective of expanding the ground-based portion of the program from 73 investigators in 1992 to over 300 ground-based investigators in 1998. As of February 8, 1995, there were 209 ground-based investigators supported by the program. Does the committee consider this target population to be adequate for the end of this decade in order to ensure the future supply of high-quality flight experiments?

Response: The committee is pleased with the direction of the ground-based program and the acknowledgment by MSAD leadership that the Research and Analysis program provides the intellectual underpinning of the microgravity program. The committee believes that the target of 300 ground-based investigators is adequate to ensure a reasonable supply of quality investigations for future flight opportunities. This judgment is based in part on the significant increase in the quality of research proposals made to the MSAD program in recent years.

7. Prompt documentation of experimental results should be required and enforced. There has been considerable discussion within NASA about access to experimental data. The observational sciences have traditionally shared their data with the community almost as soon as the picture is developed. On the other hand, laboratory research data is not usually archival in its raw state. Should NASA reconsider its policy relative to microgravity science research which provides the principal investigator exclusive rights for up to one year after receipt of data in order to verify, analyze, and publish the data and the conclusion that can be drawn therefrom?

Response: The committee recognizes that the issue of archiving flight data from microgravity experiments is extremely important and timely. This subject is therefore being addressed in an upcoming committee study.

8. There have been several comments offered that suggest that the growth of large inorganic crystals need not be a priority of this program. It would be helpful if the Committee would help in defining the term 'large' as it has several different meanings to different research groups. For example, something over 2 centimeters in diameter could be considered large, whereas industry might interpret large as 4-10 centimeters in diameter.

Response: Size, per se, is not the issue in the report's recommendations concerning the growth of large inorganic single crystals. Large in this context refers not so much to the quantitative crystal size as to the type of crystal that is the objective of the experiment. The large inorganic single crystals studied by NASA are usually grown for use

in semiconductors, detectors, oscillators, and lasers. The committee in its report stated that carrying out the growth of these large inorganic single crystals in space contributes little to the fundamental understanding of crystal growth or to improving terrestrial commercial practice.

We hope that these clarifications of the report's recommendations prove useful to you and your staff.

Signed by
Claude R. Canizares
Chair, Space Studies Board
and
Martin E. Glicksman
Chair, Committee on Microgravity Research

4.4 On Peer Review in NASA Life Sciences Programs

The Space Studies Board and the Committee on Space Biology and Medicine sent the following letter to Dr. Joan Vernikos, director of NASA's Life Sciences Division, on July 26, 1995.

The Committee on Space Biology and Medicine is pleased to respond to your letter of February 21, 1995, requesting that it evaluate the status of NASA life sciences research programs and peer review within the Human Exploration and Development of Space Strategic Enterprise in light of the recommendations of the recent NASA Federal Laboratory Review (NFLR). As you requested, the committee has focused on aspects of the organization of life sciences research within NASA and on the appropriateness of the external peer review system that the Division of Life and Biomedical Sciences put into effect in 1994. Specifically, the committee was asked to consider the following questions:

1. Are life sciences research and operational support clearly differentiated in the present organization and funding processes? Will these distinctions be clarified and accommodated by changes recommended by NFLR? Are any changes in distribution of life sciences research programs and resources within NASA that might result from implementation of NFLR recommendations likely to affect the scientific strength of the research program either positively or negatively?
2. Is research merit review being applied appropriately to both intra- and extramural research? Would implementation of NFLR recommendations strengthen or weaken merit review and thereby the scientific program? Are procedures for evaluating the effectiveness and equity of merit review currently defined and in place? Would implementation of NFLR recommendations affect these evaluations either positively or negatively?

The committee devoted its meeting of April 12-14, 1995, to these questions. Published materials available for its consideration included the NFLR report (*NASA Federal Laboratory Review*, NASA Federal Laboratory Review Task Force of the NASA Advisory Council, February 1995) and the recent letter (March 29, 1995) from the Space Studies Board to Dr. France Cordova on the role of NASA scientists and alternative management models for the science enterprise. In addition, the committee heard presentations from you, Dr. Frank Sulzman, Dr. Earl Ferguson, and Dr. Harry Holloway on the current organization of life sciences research and operational support programs within NASA and issues arising out of ongoing restructuring and downsizing of NASA; Dr. Ron White presented a detailed analysis of the results of the newly implemented 1994 external peer review process.

This letter summarizes the committee's discussion and conclusions on the above issues. The committee has not been able to fully address all of the questions raised in your letter. Rather than directly assessing specific recommendations contained in the NFLR report, the committee preferred to treat the issues underlying the NFLR, independent of the various possible interpretations of the NFLR recommendations themselves. Those issues involved the appropriate use of peer review for NASA life sciences research, as well as potential organizational and administrative changes for NASA life sciences research. In addition, your need for a prompt response in light of the rapid evolution of the restructuring process precluded a more extensive study of some of the issues. Recognizing that it has not commented on many detailed matters treated in the NFLR report, the committee hopes that the following observations on some of the major issues will be helpful to you.

Organizational and Administrative Issues for NASA Life Sciences Research

Organizational differentiation between life sciences research and operational support depends on a clear understanding of the terms. The committee applied the following definitions to distinguish among fundamental research, operational (strategic) research, and operational support:

- *Fundamental research* studies ways in which gravity or the space environment affects living organisms, including humans, and seeks to understand the basic mechanisms underlying such effects. Fundamental research is generally best addressed by investigator-initiated proposals drawn from the entire national and international research community, both within and outside of NASA.
- *Operational (strategic) research* in the life sciences addresses problems related to the presence of humans in space and their short- and long-term ability to survive and function in that environment. In many instances, operational research may be performed most effectively at NASA centers.

- *Operational support* consists of the existing technology that is necessary for spaceflight missions and includes in-house operations necessary for research experiments to be conducted in space. Operational support is necessarily concentrated at NASA centers.

By these definitions, life sciences research and operational support are not always clearly differentiated in NASA's organizational and management structure. Blurring is especially evident with respect to operational research, which arises from specific operational needs and seeks to answer operational questions or solve operational problems. The problem arises when a clear distinction is not maintained between operational research and operational support for NASA life sciences, with the result that research activities are inadequately reviewed as support activities. Careful design and conduct of such research are essential if meaningful data are to be obtained (particularly when the experiments must be carried out in space). The committee believes that rigorous peer review is the best way to assess operational research protocols and guarantee that quality and benefit are maximized. Further, it cannot be expected that the limited number of center-based NASA life scientists can include all areas of expertise that may be required to address the full spectrum of operational research problems. Moreover, additional downsizing of the intramural scientific work force, likely to result from stringent budget constraints, can only increase the dependence of NASA centers on the external scientific community.

The committee recognizes the imperative to downsize NASA headquarters and decentralize aspects of program management but is convinced that strong centralized planning, coordination, and oversight of all NASA life sciences research will continue to be necessary to ensure quality and cost-effectiveness, to facilitate advantageous interactions among centers, and to minimize potential redundancies in center programs. The committee is particularly concerned about transfer of elements of program management such as project selection to centers whose in-house programs include life sciences research. On-site science program managers charged with making funding decisions affecting both in-house NASA scientists and external applicants would inevitably be subject to conflicts of interest that could seriously damage the credibility of the selection process and the relationship with the larger external research community.

The committee also notes that life sciences research is a major priority for the International Space Station Alpha (ISSA). Effective establishment of research goals and priorities and coordination of research efforts across the entire research community are essential to prepare for the new opportunities and for efficient exploitation of those opportunities. Core expertise in the space life sciences resides in both the intra- and extramural scientific communities; neither is adequate alone. The planning and coordination required to set and meet research goals for ISSA utilization are best accomplished centrally, especially given the international dimensions of the scientific enterprise aboard the space station.

NASA centers should to the extent possible develop and maintain focus and coherence in their intramural research programs. However, the essential role of NASA scientists as the interface between external investigators and mission development and operations may impose requirements for a breadth of expertise that works against development of a critical mass in any given subfield of research. The committee supports recommendations that Ames Research Center (ARC) explore means of providing a broader intellectual environment for NASA scientists. The committee adds a similar recommendation for Johnson Space Center (JSC).

Peer Review of Life Sciences Research

All life sciences research in NASA—whether intramural or extramural, fundamental or operational—will best serve NASA's mission and scientific goals if it is of the highest quality. The committee believes the time-tested process of external peer review is by far the best mechanism for carrying out merit review in order to ensure consistent scientific excellence. Applying the same peer review process to in-house and extramural research is also important to maintaining the credibility of intramural NASA research in life sciences and the respect of the extramural scientific community.

Properly constituted external peer review does not in itself constitute a threat to the integrity of core intramural research programs and resources. The committee defines as "core" (1) the research, conducted at centers and primarily of the operational research type, that is essential to accomplishing the goals of the center's mission and strategic plan; and (2) unique facilities and resources that are necessary for carrying out NASA-supported research activities, either intra- or extramural. Clearly, panels designated to review proposals concerned with operational research questions and with experiments to be carried out in space should include appropriate expertise, and much of the necessary practical experience and expertise will be found among the scientists at NASA. The committee

sees no reasons based on intractable conflict of interest or other considerations that would preclude expert NASA scientists from serving on peer review panels together with their extramural colleagues, as long as appropriate guidelines for conflict of interest were observed. (For instance, NASA scientists would be unable to serve on a panel reviewing their own work.) In addition to the valuable practical perspective that NASA scientists would bring to proposal evaluations, such service would no doubt be a positive factor in gaining full acceptance of external peer review by the intramural community of NASA life scientists.

Unique core facilities and resources at NASA centers and other sites are important to extramural as well as intramural research activities and as such are an important focus of interaction between NASA life scientists and their external colleagues. To ensure their optimal utility as research resources for the entire life sciences community, such facilities should also receive periodic peer review including both external and internal users as reviewers. Especially in times of budgetary constraint and downsizing, questions regarding the continued effectiveness and ultimate lifespan of technological support facilities should be addressed by hard-headed examination and the broadest possible input.

Although a significant amount of NASA-sponsored life sciences research is supported outside the Office of Life and Microgravity Sciences and Applications (OLMSA), the committee has little information about the nature of these programs or current mechanisms and criteria for evaluation and selection of this research. The committee believes, however, that the same principles of external peer review should be applied to all NASA life sciences research whatever the specific program of origin.

Criteria and mechanisms should be developed for evaluating both the ongoing operations of peer review and the long-range efficacy of the system in fostering excellence in research in space biology and medicine. Appropriate criteria and procedures appear to be in place for evaluation of the OLMSA's new peer review process for life sciences with respect to equity and efficiency of operation. These include detailed analyses of scores and funding success rates on the basis of applicant demographics and solicitation of and response to feedback from applicants as well as review panel chairs and panel members. The committee was very favorably impressed by data summarizing the initial 1994 experience with the new process, which gave strong evidence that the system was equitable and effective in its operation and was being applied appropriately to both intramural and extramural research proposals. The committee strongly supports continuation of the OLMSA's new peer review process. Continued effort should be directed to shortening the time from submission of proposals to their review and especially to reducing the interval between review and final funding decisions.

Evaluation of the long-range efficacy of the current peer review system in fostering scientific excellence must necessarily await accumulation of sufficient experience over time to judge final outcomes. The committee suggests that a minimum of 3 to 5 years will be necessary to permit meaningful conclusions to be drawn. Development of useful criteria and mechanisms for analysis of outcomes is often a complex and difficult process, but the experience of the National Institutes of Health and the National Science Foundation suggests that it will be important to identify appropriate criteria as soon as possible in order to be able to collect data appropriate to the desired analyses. Possible criteria include, for fundamental research, publication of results in peer-reviewed journals of accepted quality and analysis of impact as indicated by frequency of citation and other means. Criteria for evaluating operational research should assess the impact of the research findings on operational problems, for example, improvement of protocols and procedures for flight, improvement in physiological responses of astronauts to the space environment, achievement of spin-offs, or improvement in the cost-effectiveness of operations.

The committee wishes to thank the NASA personnel who provided the information used in this review. The committee hopes that the above guidance will be useful to you in the coming months as NASA continues its restructuring and streamlining plans.

Signed by
Claude R. Canizares
Chair, Space Studies Board
and
Mary Jane Osborn
Chair, Committee on Space Biology and Medicine

4.5 On the Establishment of Science Institutes

On August 11, 1995, the Space Studies Board sent the following letter to NASA Chief Scientist France Cordova.

The Space Studies Board is pleased to respond to your request of June 8, 1995, for comments on several issues related to NASA's proposed concept of establishing science institutes as part of its Zero Base Review. You requested a rapid response with our initial comments in order to meet your schedule for further definition of the concept and the possible establishment of pilot institutes.

Your presentation to the Board during our meeting of June 8, together with some background material mailed earlier to all members, was the starting point for our deliberations on this topic. Our discussions continued on the following day with the Associate Administrators for Space Science and Mission to Planet Earth and the Deputy Associate Administrator for Life and Microgravity Sciences and Applications. A subset of the Board, together with members of the Future of Space Science (FOSS) Steering Group, also had the opportunity to discuss the proposed institutes with the Administrator, Mr. Daniel Goldin.

Your written request asked for input on three points, which I summarize here: (1) the institute concept and the conditions under which institutes could meet the stated goals of "strengthening the quality of NASA's science and expanding communication and cooperation with the external community (academia and industry)"; (2) the makeup of NASA's proposed "Institute Framework Team" and additional issues it should consider; and (3) lessons learned by the community from its experience with other, existing research institutes.

Given the need for a rapid response, this letter focuses on the first two points, although some of the Board's response is necessarily shaped by the combined experience of our members with existing institutes, as requested in point (3). In addition to space scientists, the members present during our discussions included individuals with experience with Defense Department and industrial laboratories. This response draws on the Board's assessment of the roles and missions of NASA center scientists contained in my letter to you of March 29, 1995 (the Center Science Letter). Please note that the following observations are based on our understanding of ideas and plans still in a seminal state, with many important details not yet filled in.

At the most general level, the Board believes that the formation of science institutes, under the management of external academic or industrial research entities, and for some carefully selected portions of NASA science, may contribute to the stated goals. It will be a challenge to NASA management, to the affected centers, and to their non-government partners to ensure that the adopted structures and processes achieve the goals stated in your letter, namely, to strengthen the quality of NASA's science and to expand communication and cooperation with the external community. The Board assumes that any plan for establishing science institutes would be part of a larger science plan that considers how national space research goals will be met by the sum of NASA's science activities, including both civil service and non-civil service components. Key elements of this plan would be charters for each institute that are broad enough to permit the institutes to take advantage of their independence from NASA but focused enough to implement their assigned roles in the overall science plan. These charters should be customized to each institute, and there must be incentives for each institute to adhere to its charter. Planning should also reflect a realistic appraisal of prospects for future funding (especially from non-NASA sources) for institute activities.

The Board's Center Science Letter states that the most important mission of NASA scientists is to "bind NASA's immense engineering and technical capabilities to the still larger and more diverse industrial and academic research communities across the country and the world." It further states that "this binding requires that NASA have world-class scientists who, as a group, combine both . . . internal and external functional roles . . . and are sufficiently tightly integrated into NASA's engineering and technical infrastructure." That letter identifies key examples of external and internal functions for NASA scientists and then describes four principles or qualities of NASA science that would support the stated mission. In brief, these qualities are (i) scientific excellence and depth, (ii) sufficient scientific breadth, (iii) firm integration into NASA's technical and engineering infrastructure, and (iv) interdependency among NASA centers and with the external community.

Certain internal and external functions described in the Center Science Letter, such as participation in policy formulation and selection of external investigators, are properly the province of government employees, but should not be vested in field centers in order to avoid real or perceived conflicts of interest vis-à-vis outside scientific competitors. It is therefore the recommendation of the Board that these functions be retained by Headquarters, where they would be discharged by government employees.

Considering the proposed institutes in terms of the four principles or qualities presented in the Center Science Letter, the Board offers the following observations and recommendations:

(i) **SCIENTIFIC EXCELLENCE.** The major motivation given for establishing science institutes is to enhance scientific excellence. The Board believes that a proper institute structure could well contribute to this goal. Process is also important: plans should be openly developed, widely understood, and methodically and consistently implemented. Otherwise, uncertainties and turmoil during the transition could degrade current scientific quality by driving the best (and therefore most employable) scientists out of NASA and its research programs.

(ii) **SCIENTIFIC BREADTH.** Institutes with well-defined charters could fit into an overall NASA science activity that meets the agency's requirements for breadth across the relevant disciplines. It is unclear whether interdisciplinary research, a valued by-product of scientific breadth, would be better enabled at the proposed institutes than in-house at the centers.

(iii) **INTEGRATION.** Achieving tight integration into the NASA engineering and technical infrastructure may prove more difficult for external institute personnel than for in-house civil service scientists. At least in the pilot institutes under discussion, the scientific activities to be collected in external institutes are not the main focus of their parent centers. In such cases, where the science programs may be less reliant on the primary technical infrastructure of the parent center, the need for, and potential benefit from, tight integration are reduced. On the other hand, many of the functions identified in the Board's Center Science Letter entail field center scientists strongly influencing or even directing activities in key engineering and technical areas. Where institute scientists are expected to exercise these functions but are viewed as "contractors," those roles could be compromised. It might be useful to find existing examples where non-government scientists have successfully taken leadership roles in relation to a government laboratory. (The Jet Propulsion Laboratory is not such a case, since there the entire center is staffed with non-civil servants.)

(iv) **INTERDEPENDENCY.** Greater interdependence between centers and the outside community might be achieved if the institutes can maintain firm ties to both. It is less clear how institutes would strengthen interdependency among centers or work to soften a center's insularity or defensive posture. The Center Science Letter recommends that "NASA should strive to assure that the centers themselves and their senior managers assume greater responsibility for a healthy partnership with the external industrial and university community." Formation of institutes should not be allowed to diminish this ongoing responsibility.

With respect to your second point, the composition of the "Institute Framework Team," the Board strongly supports the suggestion that such a team have vigorous external participation. Any plan for establishing institutes will stand or fall on its details, and we have provided some issues for the Team's consideration. Independent perspectives from outside NASA should have an important role formulating those details and addressing these issues.

As you know, the FOSS study is addressing science organization within NASA in a more comprehensive manner, including the question of science institutes. The final report of the FOSS study will include consideration of your point (3). Every attempt is being made to expedite completion of this report, as Mr. Goldin requested, and we hope that it will help make a significant contribution to the NASA reinvention process.

We hope that these brief comments are helpful and look forward to additional discussions on these important issues at future meetings.

Signed by
Claude R. Canizares
Chair, Space Studies Board

4.6 On “Concurrence” and the Role of the NASA Chief Scientist

On December 12, 1995, the Space Studies Board and the Committee on the Future of Space Science sent the following letter to NASA Chief Scientist France Cordova.

On behalf of our colleagues, we wish to thank you and your associates in NASA management for the detailed and thoughtful response you are developing to the recent report of the Space Studies Board’s Committee on the Future of Space Science, entitled *Managing the Space Sciences*. It appears from these draft responses and from the discussion of them at the Board meeting on November 28 that the report uses the term “concurrence” in a way that has led to some misunderstanding. This letter is intended to clarify the report’s recommendation that the NASA Chief Scientist be given formal concurrence authority with respect to matters of science budgets, programs, and plans.

In the report, “concurrence” is not meant to imply an additional level of line management interposed between the NASA Administrator and the science associate administrators. On the contrary, the intent of the report is that the associate administrators will continue to present their plans, budgets, and programs directly to the Administrator, as at present. They will report, as now, directly to the Administrator.

The report’s recommendation on concurrence does propose the establishment of a formal procedure whereby, at presentations of plans and budgets by the science associate administrators, the Administrator would ask the Chief Scientist for a position on what has been proposed. The expectation would be that if the Chief Scientist did not support a particular proposal, he or she would explain why not. Of course, the Administrator would be completely free, as he is now, to overrule the views of the Chief Scientist (as he is also free to reject proposals made by senior line management). In practice, the Chief Scientist consults with the associate administrators throughout the process of developing plans and budgets, and the great majority of potential sources of disagreement and nonconcurrence would be resolved long before they reach the Administrator.

The committee and Board believe that the proposed concurrence mechanism corresponds fairly well to present, but informal, practice. The committee’s report suggests, however, that this mechanism, if formalized and adopted by the Administrator, would strengthen the coordinating role that the Chief Scientist is expected to perform across the agency’s science programs. The committee and Board further expect that formalizing the concurrence function of the Chief Scientist would increase the appeal of that position to highly qualified candidates in the future.

Members of our committee would be pleased to meet with NASA officials to discuss this or other recommendations of the report; such a meeting might provide a good opportunity, in particular, to explore the report’s recommendations on the research prioritization process.

Signed by
Claude R. Canizares
Chair, Space Studies Board
and
John A. Armstrong
Chair, Committee on the Future of Space Science

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